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THE GROUP AS A STIMULUS TO MENTAL ACTIVITY[1]

THE purpose of this paper is not to present the results of an original investigation, but merely to suggest a problem. Efficiency in brain activity and in correlated mental activity depends upon many conditions. Among these are physiological age, race, sex, the blood supply to the brain, as determined by general nutrition, exercise, posture, and the size of the cerebral arteries; the quality of the blood as determined by food, drugs, the supply of oxygen, nasal respiration, etc.; again by a group of conditions which make up the environment, the temperature, humidity, barometric pressure, light, peripheral stimulation, etc. Again as the social instincts in man are fundamental, one of the most important factors in his environment is the presence or absence of other human beings. This can not be ignored. The problem I wish to present is this: What is the effect on mental activity of the presence of a group of other persons, if studied objectively like the effects of temperature, barometric pressure, or the like? Perhaps the best way to present this problem is to recount briefly the meager but important results of investigations already made.[2]

Studies in social psychology have shown that an individual alone and the same individual in a group are two different psychological beings. Recent investigations show that the same is true of children. The

[1] Read before Section L, American Association for the Advancement of Science, Boston, December, 1909.

[2] For reference to the studies mentioned below see *Ped. Sem.*, Vol. XII., June, 1905, pp. 229–230.

child working alone is different from the child working in a class. A few years ago Dr. Mayer, of Würzburg, studied experimentally this difference as regards the ability to do school work. His problem was to determine whether and under what conditions the work of pupils in a group give better results than the individual work of the isolated pupil. He tested the ability of pupils to work alone or in company with others, using dictation, mental arithmetic, memory tests, combination tests after the manner of Ebbinghaus, and written arithmetic.

Dr. Mayer's method was briefly as follows: a number of boys in the fifth school year of the people's school in Würzburg were given five different tasks as class exercises, and also each boy was required to prepare a similar task for comparison in which he sat alone in the class-room, only the class teacher or a colleague being present. The material for the tasks was carefully chosen and was familiar to the pupils. The pupils were representative of very different elements as regards school ability, behavior, temperament, and home conditions. The number tested was 28, the average age twelve years.

In general the result of the work of the pupils in groups was superior to their work as individuals. This appeared not only in the decrease of time, but in the superior quality of the work done. In individual cases the saving of time was specially striking; for example, one pupil for a combination test required 10 minutes and 25 seconds when working alone, for a similar test when working with the group 7 minutes and 30 seconds; another, alone 13 minutes and 11 seconds, with the group 6 minutes and 45 seconds.

Dr. Triplett tested the influence of the presence of a coworker on a simple physical performance. His subjects were forty school children, and he had them turn a reel as rapidly as possible. The children turned the reel now alone and then in company with another child, in both cases with directions to turn as rapidly as possible. Two results were noted. It appeared, on the one hand, that pupils worked more rapidly when another child worked in combination; but, on the other hand, in case of many children, hasty uncoordinated movements appeared which reduced their performance.

Wherever men are together the individual is influenced by others without being aware of it. This is specially well illustrated by certain experiments in the laboratory. Meumann cites the case of a subject whose work at night with the ergograph had a very definite value. Accidentally one evening Meumann entered the laboratory, and at once the work done was decidedly increased in comparison with that of other days, and this without the subject's making any voluntary effort to accomplish more. In such experiments the subject always attempts to do his utmost, and hence the significance of the increased work done in the presence of another individual. Many examples of such effects of suggestion have been reported by psychologists.

Meumann, in experiments in the People's Schools, corroborated the results of Triplett and Féré in a striking manner. Seven pupils of the age of thirteen or fourteen were tested repeatedly with the dynamometer and ergograph. In case of the test of the pupils separately, with no one else in the room, the amount of work was always less than when others were present. If the experiments were made in the presence of the teacher alone, the pupils did not do as much work as when they were all together without the teacher.

From all this it appears, as Mayer points

out, that pupils in a class are in a sort of mental *rapport;* they hear, see and know continually what the others are doing, and thus real class work is not a mere case of individuals working together and their performance the summation of the work of many individuals; but there is a sort of class spirit, so that in the full sense of the word one can speak of a group performance, which may be compared with an individual performance. The pupils are members of a community of workers. The individual working by himself is a different person. Schmidt in his careful investigation testing school children in their home work as compared with their school work found that for most kinds of work the product in the class-room was superior. His results are to a considerable degree evidence in corroboration of the results found by Mayer. The child studying school tasks at home is relatively isolated; in the class he is one of a social group with common aims.

A noteworthy result of these investigations is the apparent immunity of children to distraction from ordinary causes. Schmidt found that the outside disturbances—the noise from the street, from adjoining rooms, and the like, had little effect upon them. It was only interruptions that distracted their attention, such as conversation with others, that affected the quality of their work. It appeared even that a home task completed without disturbance might be poorer than the corresponding class work, and that a home task when the pupil was disturbed might be better than the class work. And from Mayer's study it appeared that the tendency to distraction is diminished rather than increased by class work.

Meumann in tests of the memory of pupils alone and when working together found similar results. Disconnected words of two syllables were used, which were written down, pronounced once to the pupils and then written down immediately by them from memory. It would naturally be supposed that the children working in the class-room, with all the inevitable noises and disturbances, would not remember as well as when tested alone. The result of Meumann's investigation, however, was surprising. While in case of children thirteen and fourteen years of age there was no essential difference in memory for the individual and the common test, the difference was remarkably large in case of the younger children, especially in case of those eight and nine years of age. On an average with the individual test the children remembered considerably less than in the class. The results were constant. Not a child was found who remembered more in the individual test than in the class test. From this Meumann concludes that the great number of disturbing influences to which children are inevitably exposed in the classroom—the noise of writing, whispering, walking about, the occasional words of the teacher, the sight of the movements of the pupils, and the like, which one might naturally suppose would make the results inferior, have no special influence.

Meumann asked a number of the pupils in case of the individual tests whether they would prefer to take such exercise in the class or alone, whether they were disturbed by the noise of the other pupils. To his surprise 80 per cent. of the pupils gave the decided answer that they would prefer to do the work in the class. Some 15 per cent. gave no definite answer. The others, an extremely small minority, replied that they were disturbed in the class-room; and in most cases these were sensitive, nervous or weak children, although among them were some individuals of decided talent.

Thus it appears that the presence of a

group distinctly affects the mental activity. Of course the easy explanation of the increased ability to work often found in the group is to say that it is due to ambition. rivalry and the like. This is all true enough, but we can analyze this a little further.

A few things are pretty obvious. First of all, where activity is involved, there is the stimulus to greater exertion which comes from the sight of another performing an act. As Professor James has said, the sight of action in another is the greatest stimulus to action by ourselves. This has manifold illustrations from the activities of primitive man to modern experiments in the laboratory. In early stages, for example, an institution sometimes found is the *præsul*. A leader stands before a group who are engaged in work or a dance and himself performs perhaps in pantomine the activities which they are attempting. This stimulates and renders easier the activity of the group. Every paced race on the athletic field also furnishes an excellent illustration. Again in the laboratory, Féré found that the amount of work one could do with the ergograph was increased by having another person simply go through the action of contracting the muscles of the finger in sight of the subject of the experiment, the second person acting as a sort of pace-maker for the first. The clearer and more intense the idea of an action the more efficient the action.

There is undoubtedly also an affective stimulus in the presence of the group. This is the stimulus which comes from our social impulses as inherited from the past, and yet it should be noticed that such affective stimuli, which, I take it, are what is really meant by ambition and the like, may act either to increase or to inhibit the mental activity. A certain degree of affective stimulus undoubtedly increases the ability to work, but if the stimulus is extreme the work is checked or inhibited altogether. For example, extreme anger, stage fright and even extreme joy, in the presence of the group, may inhibit the mental activity.

In many individuals at least the presence of the group is a stimulus to greater concentration of attention. In case others are doing the same thing, this helps us attend better to the activity in hand; and even in case others are doing something different, the distraction itself is sometimes a stimulus to better attention, because the individual tries to resist the distraction and there is an over-compensation which improves the attention. Meumann, for example, has found this result in certain experiments.

Meumann emphasizes particularly this compensation power of attention. Not merely is it true that the performance of an individual often increases when there are disturbing stimuli, because the increased concentration to overcome the distractions increases the work; but more than this, the compensation, which in this case becomes an over-compensation, shows that the disturbing stimulus has the effect of increasing rather than decreasing the energy, that is, it has a dynamogenic effect, although this effort does not occur in case of all individuals.

The measure of this is of course the increase of the performance by the distracting stimulus. This is very well shown with the distraction stimulus when one is committing to memory. By Meumann's method the memory span or the number of figures or letters that can be remembered without error after once hearing is determined, and then disturbing stimuli are introduced. An acoustic stimulus may be introduced for distraction, *e. g.*, a metro-

nome strikes. Such a distraction often improves the performance.

To describe the stimulus to the imagination from the group would be commonplace. We need not go to the laboratory nor cite the case of children for illustration. The man in the crowd has always been able to see what has happened and more besides, to foresee impending danger, or anticipate success, or hear voices from the unknown and behold inspiring visions. We need not, I think, go back to ancient history for illustrations of even the latter. A week ago in my home city thousands of people watched for mysterious lights in the heavens, and not a few saw them and knew exactly what they meant. Nor was this the only place where men saw the moving lights of airships. Even of the groups on Boston Common it was reported that the clear rays of a moon approaching the full failed to undeceive "those who, having seen, believed, or believing that they had seen refused to doubt, or not having seen, had met and talked with those who had seen, or believed they had seen or had met those who had seen."

As regards the relative merits of solitude or a social environment for scholastic pursuits I am not concerned here to speak. But the weight of evidence thus far seems to be to indicate the advantage of group work, except when individual and original thinking is required. This is perhaps one reason why the man of genius has frequently desired solitude. There are undoubtedly, also, great individual differences as regards the effect of social environment; there are even perhaps different types as regards the effectiveness of the stimuli from the social group. There may perhaps be one type that does its best work in solitude, another type that does its best work in the group. This again is one of the problems that should be investigated.

Again, of course, the question is relative to the kind of work done. Mayer's experiments indicate that for some kinds of work the stimulus of the social group is needed. For some kinds of work, especially where original thinking is demanded, the environment of solitude is better.

What we may call the social stimulus to mental activity is such a commonplace matter that probably very few realize its significance. When, however, we recall the fundamental character of our social instincts it is not strange that the presence of other people should be a most potent stimulus either increasing or checking the mental activity. Psychologists have always recognized the fundamental character of the stimulus from ambition, rivalry and the like. But this social stimulus goes much farther back and is rooted in the reflexes of the sympathetic nervous system that are correlated with emotion. This is well illustrated in experiments with animals. Mosso found in his experiments testing directly the sympathetic reflexes in the dog that the presence of the master in the room at once affected the reflexes; and Dr. Yerkes, of Harvard University, finds that in his experiments with dogs the presence of the experimenter is always likely to affect the results.

The fundamental character of the social stimulus is shown also in many fields of human activity according to one view of esthetics. The artist always works with the audience in his mind. The teacher also and the orator are apt to do much of their work with the class or audience in mind. I am not concerned here with the fact that this often becomes a grotesque and exaggerated mark of the profession but merely with this as an illustration of the fundamental character of what we have called the social stimulus.

In fact this social stimulus colors every-

thing. It is comparable only to the constant peripheral stimulation which is necessary to keep us awake; in like manner a social stimulus is necessary as an internal condition, as we may say, of consciousness.

Perhaps the fundamental character of this social stimulus is seen best in the case of persons who are in solitude. The pathetic devices of prisoners, for example, their custom of making pets of mice, flies, or anything found in their cells, and their interest in any form of activity—all these are attempts to make some symbolic substitute of activities having social value for the lack of direct social stimulus. The making of things having a social value seems to appeal to them.

Griffith, for example, says that solitary confinement is "so good an instructor that very little time is needed for teaching prisoners a trade. They go to work without squares, gravers, stamps, patterns or models. Every scrap of glass or metal, every nail and pin turns to account as a tool. Waste from the shop, bones from the kitchen, walnut, cocoanut and acorn shells," etc., serve as materials.[3] But this along with many other pathetic devices to which prisoners resort are means of saving them from the misery of solitude. This does not seem due entirely to the satisfaction of the instinct of activity, but in part to the satisfaction given symbolically to the social instincts.

The social instincts are so strong in children that if they are so unfortunate as to be largely isolated from others they are apt to create imaginary companions and to live in a dream world of society.

The aim of this paper is to present the problem. Let me for a moment, however, hint at a wider point of view.

The investigations referred to have chiefly concerned the mere presence or absence of other individuals performing similar tasks. In a true social group the relations are more vital. Each individual feels a responsibility and performs some service for the group. Here the stimulus is likely to be greater. Perhaps the greatest stimulus to mental activity from the group is social success to those who can achieve it.

Both experiment and observation have shown the great stimulus resulting from success in general. Social beings that we are, no form of success is so stimulating as a social success. When we reflect that under present conditions many of the children in our schools are so placed that a social success is impossible we see the significance of this point.

Not to mention the frequent domination of the class group by the teacher and the artificial relations often existing in our school recitations, as shown so vividly by Dr. Scott, the many defects of school children shown by modern studies in school hygiene often make social success impossible.

Among the pathetic tragedies of childhood are the cases of those who never can achieve success because of defect—the child with defective vision who can not see the blackboard, the deaf child who can not hear the teacher, the child tormented with headache or toothache, the child whose brain nutrition is reduced by nasal obstructions, the sensitive child, the misunderstood child, and the whole list of nervous defectives.

An important relation between the development and integrity of the sense organs and mental efficiency has been shown by a number of investigations. A large

[3] Small, Maurice H., "On some Psychical Relations of Society and Solitude," *Pedagogical Seminary*, April, 1900, Vol. 7, No. 1, pp. 13–69.

per cent. of those children who have defective hearing have often been found to be dullards. Also those suffering from adenoid growths are likely to be found in the class of dull children. And while myopic children are often found among those more precocious and studious in school work, this due, perhaps, to their lack of normal interest in things out of doors and muscular activities, those with eye defects often seem hopelessly dull.

It is evident that we are dealing with a problem fundamental in pedagogy and school hygiene. Every parent knows the leaden stupidity that at times comes over children, and every teacher has doubtless had experiences with at least a few cases of it in chronic form. This is the one defect which to many teachers seems hopeless. The only redeeming thing about stupidity seems to have been discovered by a German, who with rather a labored attempt at wit has said that the stupid children will make invincible soldiers, because the gods themselves fight in vain against stupidity; but what is impossible to the gods of pedagogy is sometimes possible to Hygeia. When stupidity is due to a defect of the sense organs, the difficulty can sometimes be removed by the simple device of seating the pupil in a favorable position; a surgical operation for an adenoid growth has removed the cause of stupidity in the case of many children; and frequently what the stupid child specially needs is enough to eat, or sufficient sleep, or rest from work imposed out of school hours, or perhaps the mere stimulus of social success. In any case the cause should be sought.

Thus the simple problem with which we started leads out into the wider problems of social hygiene and social pedagogy; and here I must leave it with the hope that it will be considered by teachers and studied further by investigators.

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THE PRINCIPLE OF RELATIVITY

AT the recent Boston meeting of the American Physical Society there was so much general interest in the principle of relativity and so many questions were asked me personally by those who had given the subject very little attention, that it seems timely to give a brief introduction to the subject on a somewhat simpler basis than I think has yet been attempted. The method employs several of the "non-mathematical" conceptions first introduced by Lewis and Tolman, but I think the demonstrations will be found even simpler than theirs.

The principle of relativity is one attempt, and by far the most successful attempt as yet, to explain the failure of all experiments designed to detect the earth's motion through space, by its effect on terrestrial phenomena. It generalizes this universal negative result into its first postulate, which is, *the uniform translatory motion of any system can not be detected by an observer traveling with the system and making observations on it alone.*

The second postulate is that *the velocity of light is independent of the relative velocity of the source of light and observer.*

At the very outset, it is important to realize that we have no long-standing experience with systems moving with velocities comparable with that of light, and therefore that primitive intuition may not be the very best guide in first introducing us to them. We might easily imagine a peasant scorning the suggestion that the dimensions of a rigid body changed with the temperature, and declaring, on being

pressed that such an idea was clearly against common sense.

The whole principle of relativity may be based on an answer to the question: When are two events which happen at some distance from each other to be considered simultaneous? The answer, "When they happen at the same time," only shifts the problem. The question is, how can we make two events happen at the same time when there is a considerable distance between them.

Most people will, I think, agree that one of the very best practical and simple ways would be to send a signal to each point from a point half-way between them. The velocity with which signals travel through space is of course the characteristic "space velocity," the velocity of light.

Two clocks, one at A and the other at B, can therefore be set running in unison by means of a light signal sent to each from a place midway between them.

Now suppose both clock A and clock B are on a kind of sidewalk or platform moving uniformly past us with velocity v. In Fig. 1 (2) is the moving platform and (1) is the fixed one, on which we consider ourselves placed. Since the observer on platform (2) is moving uniformly he can have no reason to consider himself moving at all, and he will use just the method we have indicated to set his two clocks A and B in unison. He will, that is,

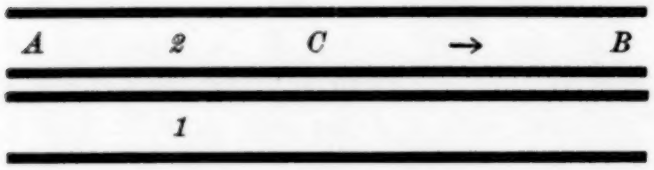

Fig. 1

send a light flash from C, the point midway between A and B, and when this flash reaches the two clocks he will start them with the same reading.

To us on the fixed platform, however, it will of course be evident that the clock B is really a little behind clock A, for, since the whole system is moving in the direction of the arrow, light will take longer to go from C to B than from C to A. Thus the clock on the moving platform which leads the other will be behind in time.

Now it is very important to see that the two clocks *are in unison for the observer moving with them* (in the only sense in which the word "unison" has any meaning for him), for if we adopt the first postulate of relativity, there is no way in which he can know that he is moving. In other words, *he has just as much fundamental right to consider himself stationary as we have to consider ourselves stationary,* and therefore just as much right to apply the midway signal method to set his clocks in unison as we have in the setting of our "stationary clocks." "Stationary" is, therefore, a relative term and anything which we can say about the moving system dependent on its motion, can with absolutely equal right be said by the moving observer about our system.

We are, therefore, forced to the conclusion that, unless we discard one of the two relativity postulates, the simultaneity of two distant events means a different thing to two different observers if they are moving with respect to each other.

The fact that the moving observer disagrees with us as to the reading of his two clocks as well as to the reading of two similar clocks on *our* "stationary" platform, gives us a complete basis for all other differences due to point of view.

A very simple calculation will show that the difference in time between the two moving clocks is[1]

[1] The time it takes light to go from C to B is $\frac{1}{2}/(V - v)$ and the time to go from C to A is $\frac{1}{2}/(V + v)$. The difference in these two times is the amount by which the clocks disagree and this

$$1/V\ \beta/(1-\beta^2)$$

where

l = distance between clocks A and B;
v = velocity of moving system;
V = velocity of light;
$\beta = v/V$.

The way in which this difference of opinion with regard to time between the moving observer and ourselves leads to a difference of opinion with regard to length also may very easily be indicated as follows:

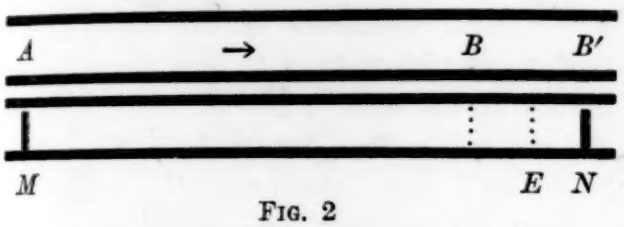

FIG. 2

Suppose the moving observer desires to let us know the *distance* between his clocks and says he will have an assistant stationed at each clock and each of these, at a given instant, is to make a black line on our platform. He will, therefore, he says, be able to leave marked on our platform an exact measure of the length between his clocks and we can then compare it at leisure with any standard we choose to apply.

We, however, object to this measure left with us, on the ground that the two assistants *did not make their marks simultaneously* and hence the marks left on our platform do not, we say, represent truly the distance between his clocks. The difference is readily shown in Fig. 2, where M represents the black mark made on our platform at a certain time by the assistant at A, and N that made by the assistant at B at a later time. The latter assistant waited, we say, until his clock read the same as clock A, waited, that is, until B was at B'; and then made the mark N. The moving observer declares, therefore, that the distance MN is equal to the distance AB, while we say that MN is greater than AB.

Again it must be emphasized that, because of the first fundamental postulate, there is no universal standard to be applied in settling such a difference of opinion. Neither the standpoint of the "moving" observer nor our standpoint is wrong. The two merely represent two different sides of reality. Any one could ask: What is the "true" length of a metal rod? Two observers working at different temperatures come to different conclusions as to the "true length." Both are right. It depends on what is meant by "true." Again, asking a question which might have been asked centuries ago, is a man walking toward the stern of an eastbound ship really moving west? We must answer "that depends" and we must have knowledge of the questioner's view-point before we can answer yes or no.

A similar distinction emerges from the principle of relativity. What is the distance between the two clocks? Answer: that depends. Are we to consider ourselves with the clock system when we answer, or passing the clocks with a hundredth the velocity of light or passing the clocks with a tenth the velocity of light? The answer in each case must be different, but in each case may be true.

It must be remembered that the results of the principle of relativity are as true and no truer than its postulates. *If future experience bears out these postulates then the length of the body, even of a geometrical line, in fact the very meaning of "length," depends on the point of view, that is, on the relative motion of the observer and the object measured.* The reason this conclusion seems at first contrary to common sense is doubtless because we, as a race, have never had occasion to observe directly velocities high enough to

difference becomes, on simplification, the expression given above.

make such effects sensible. The velocities which occur in some of the newly investigated domains of physics are just as new and outside our former experience as the fifth dimension.

Returning now to the magnitude of this difference of opinion as to the distance between the clocks, it is easy to show that, from our point of view, the moving observer overestimates the distance in the ratio

$$1/(1-\beta^2).$$

So that it may be said in general that lengths in the direction of motion, which *he* says are equal, *we* say are unequal in this same ratio.

On lengths perpendicular to the direction of motion our estimates agree.

Now let us ask ourselves: What are *corresponding lengths* in the two systems? Corresponding lengths may with propriety be given the same name, "meter" for instance. The condition that two lengths should be "corresponding" is simply that each observer comes to the same conclusion with respect to the *other* length.

The lengths AB and MN are not "corresponding," for the moving observer says that MN is *equal* to AB, while we say AB is *less* than MN, in the ratio $(1-\beta^2)$. If, however, we mark off on our platform a length which is a mean proportion between our estimate of the length AB and the length MN, this length, say ME, will "correspond" to the length AB, for we shall then say, that AB is less than ME in the ratio $\sqrt{1-\beta^2}$, while the moving observer will say that ME is less than AB in the same ratio.

Thus any length, in the direction of motion, on a moving system is estimated less in the ratio $\sqrt{1-\beta^2}$ by a "stationary" observer.

Or, put in a better way, *an observer viewing a system which is moving with respect to him, sees all lengths, in the direction of motion, shrunken in the proportion* $\sqrt{1-\beta^2}$, *where* β *is velocity with which the system is passing him in terms of the velocity of light.*

We have now reached two results, which we may summarize thus; first, clocks which a moving observer calls in unison do not appear in unison to a "stationary" observer, the clock in advance as regards motion appearing behind the other in time, and second, distances in the moving system appear shortened in the direction of motion in the ratio $\sqrt{1-\beta^2}$. In the above we can, of course, interchange the words "moving" and "stationary."

Next let us turn our attention to the unit of time in each system. *It is not hard to show that the unit of time in the moving system will appear to us greater than ours in the ratio* $1/\sqrt{1-\beta^2}$. This is due to the fact that in the moving system forward clocks are behind in time.

In the measurement of time we assume a certain standard motion to be taking place at a constant rate and then take as a measure of time the total displacement which this motion has caused. Time measurement with an ordinary clock is obviously a special case of this general rule.

The moving observer can adopt as his unit of time the time it takes light, moving with the characteristic[2] space velocity V, to travel a certain distance d and return to him.

Suppose d is in the direction of motion, and the light after traveling a certain distance in the direction of motion is reflected back to the observer. He will then write

$$t = d/V.$$

We, however, "know" that he is overestimating the distance d in the ratio $1/\sqrt{1-\beta^2}$

[2] That the moving observer's estimate of V can not change with his velocity follows of course from the first postulate.

and overestimating also the average velocity with which his signal *travels through his system* in the ratio $1/(1-\beta^2)$,[3] thus he is *under*estimating his time in the ratio $\sqrt{1-\beta^2}$. A certain time interval, that is, appears less to him than to us and hence his unit of time appears to us *greater* than ours in the ratio $1/\sqrt{1-\beta^2}$.

This paper has become long enough without an attempt to discuss the units of mass and force. It has been my purpose merely to answer a number of questions which the experience of the Boston meeting led me to believe were in the minds of many who had not given the subject enough thought to understand easily the more profound discussions.

The apparent transverse mass is, I think, best derived by Lewis and Tolman[4] in their excellent paper on the principle of relativity, and the relation between transverse and longitudinal mass is shown in the most direct and simple way by Bumstead[5] making use of the torsion pendulum. Any one interested in the subject should read these two papers.

It is, of course, true that the principle of relativity has a much deeper logical significance than the simple, more or less concrete conceptions on which it is based in the present paper would lead one to suppose, but in an introduction to such a subject concreteness may not be a fault.

It should be restated that the results of the principle for uniform translation are simply as true as its two postulates. If either of these postulates be proved false in the future, then the structure erected can not be true in its present form. The question is, therefore, an experimental one.

I think it may be said with fairness, however, that the principle is already in harmony with so many phenomena that the burden of proof lies with those who object to it. Besides the negative result of experiments to detect the earth's motion the principle is supported directly by the recent experiment of Bucherer,[6] and by the still more recent experiment of Hupka.[7] Indirect support is also given by Lewis's[8] independently derived theory of non-Newtonian mechanics, which agrees exactly with relativity results, and by Comstock's[9] deductions from orthodox electromagnet theory which lead to conclusions so nearly coincident with those of relativity as to be very suggestive.

In closing, a word should be said with regard to the "addition of velocities" according to relativity rules. It will be evident on a little thought that if the moving platform of Fig. 1, which is *passing us* with velocity v, has on it a body *traveling over it* in the direction of its motion with velocity v (that is, with a velocity which the observer on the moving platform calls v), then *our* estimate of the velocity *of the body* will *not* be $v + v_1$. The reason is of course that $v + v_1$ is the sum of two quantities, one of which is estimated by us and the other by the moving observer. We should, therefore, be inconsistent because we should have mixed view-points. *Our* estimate of the platform's velocity plus *our* estimate of the body's velocity with respect to the platform equals *our* estimate of the

[3] The average velocity of a signal traveling through his system with a velocity which we estimate as $V-v$ in one direction and $V+v$ in the other, is of course obtained by dividing the total distance by the total time. The total time is obviously

$t = \frac{1}{2}$ distance$/(V-v) + \frac{1}{2}$ distance$/(V+v)$,

and hence the average velocity is

$$V_a = V(1-\beta^2).$$

[4] *Phil. Mag.*, 18, 510–523, 1909.

[5] *Am. Jour. of Science*, 26, pp. 493–508, 1909.

[6] *Ann. d. Phys.*, 28, S. 513–536, 1909.

[7] *Ann. d. Phys.*, 31, S. 169–204, 1910.

[8] *Phil. Mag.*, 16, pp. 705–717, 1908.

[9] *Phil. Mag.*, 15, pp. 1–20, 1908.

body's velocity. In this last case we have stuck to one point of view and obtained a correct result.

This feature connected with the so-called "addition of velocities" is what Professor Michelson and others so strongly object to in the relativity principle, but the result is a perfectly natural one as soon as we have seen the admissibility of more than one point of view and the difference in estimates caused thereby.

D. F. Comstock

Massachusetts Institute of Technology

Some Considerations as to the Nature of Comets and their Probable Relation to the Sun

The ideas herein put forward are not all original with the author, though it is believed some of them may be. It is hoped that the considerations may, however, help to a simple rational understanding of the major facts regarding the behavior of comets.

The exceedingly high temperature of the sun causes it to be surrounded by an atmosphere of vapors. Some of the vaporized matter condenses in the outermost layers and eruptions are constantly occurring which partly fill the space around it with very fine particles, the smaller of which are repelled by the pressure of the sun's radiation, which pressure even overcomes the gravitative force of the sun itself. These ejected particles probably constitute the streamers which are visible during total eclipses as extending from the sun to immense distances. What we see is the effect of innumerable overlapping streams. Their extreme tenuity is evidenced by the comparatively feeble luminosity in spite of the great depth of the flux which we are at any time observing. This depth is, of course, greater than the diameter of the sun. Such coronal streamers are by no means uniformly distributed about the sun, but in certain directions, varying continually, may be more dense than in others, coinciding perhaps with great eruptive areas of the sun's surface. It probably happens that when the outbreak is unusually violent, and when the earth happens to be passing through that part of space occupied by an abnormally extended streamer, an aurora of greater or less intensity or duration may attend the sweeping of the earth by such a streamer. The particles are probably ions or carry electric charges, and induced auroral streamers in the earth's atmosphere are for the time being visible on its dark side away from the sun.

It has been thought that comets may act in a somewhat similar way to disclose the condition of the ejected material of the sun, or, as may be conceived, to disclose a stratification or unevenness of distribution of the ejected matter from the sun. Since there is reason to believe that much of this matter is in a highly electrified state, it is not to be doubted that electrical phenomena are at the same time produced, with accompanying evolution of light. Indeed, in the free space around the sun, there must be a great intensity of ultra-violet radiation which of itself would cause emission of negative ions from matter in its path and produce electrical disturbances. But aside from this possibility, the comet is recognized as an assemblage of particles larger or smaller, moving in an orbit which involves great variations of its distance from the sun. In passing through the depths of space far away from the sun, these parts or particles may tend, by their very feeble gravitative effect, to gather up any finer particles which, on account of the intense cold of space, are substantially solid, even though at ordinary temperatures they would be gaseous. The parts of the comet's nucleus more or less porous would in this way accumulate upon their surfaces and in their pores occluded gases, condensed material and fine dust, and there would be a period of many years in which this gathering-up process, as in the case of Donati's and other long-period comets, could occur. Let a comet as an assemblage of such small masses after its long course through remote space, during which it has gathered fine particles ejected from the sun or from other bodies, reach, in

approaching the sun, a part of its orbit where the temperature given by the solar radiation to the surfaces of the masses is sufficient to boil off or regasify the condensed material; then not only is the gas blown off into vacuous space around the nucleus of the comet, but it is naturally blown off in the direction towards the sun, from the heated side of each mass, and at the same time that the gas leaves the mass other fine particles are lifted by the force of the escaping gas. This is due to the fact that these fine or dust-like particles are not held with any strong gravitative tendency. Ultra-violet radiation may also add its effect in causing discharge of negative ions. The result of this is that jets or flows of materials from the nucleus tend into the vacuum towards the sun from the warmed or radiation absorbing surfaces of the comet's nuclear masses. As soon as they leave the nucleus or the warmed surfaces, they are again cold and mainly condensed. But, though exceedingly fine, they are now absorbers, more or less solid, of the sun's radiation, and are gradually thrust backward by the pressure of the light and radiation and are blown off in the opposite direction by this pressure, so forming a tail in the contrary direction from the sun, or in a direction opposite to that in which they were first ejected. There being in matter all grades of volatility, as the cometary body approaches the sun, material more and more refractory, so to speak, would be evolved, until finally, if the approach is near enough to the sun, even ordinarily solid substances would be vaporized from the nuclear masses and projected to form a tail, as has just been described. Some of this vaporized matter would immediately condense on getting a little farther away, and form solid particles in the tail. The comet of January, 1910, showed sodium lines, showing that the temperature of the nuclear masses had probably reached the vaporization point of sodium. The greatest extension of a comet's tail usually comes just after the comet passes perihelion, because the heating process keeps on, as it were, a little past perihelion, just as the hottest part of our summer days is two or three o'clock in the afternoon. Now if the comet stays in proximity to the sun long enough, it will have discharged nearly all of its volatile material for a particular temperature reached. But on leaving the sun after the tail has shrunk (which is a very natural thing for it to do when the body passes through regions less heated by solar rays), it may again be in the condition to gather up the condensed and practically solid gases and vapors in the space around it. And if its period is a long one, such as 2,000 years, as in the case of Donati's comet, it should not surprise us if there is sufficient material to form a fair tail, which only lasts a few weeks at the most. Then it must be borne in mind, too, that an extremely small amount of material diffused in space under solar radiation will suffice to form a very large tail, as every particle, even of extremely small mass, becomes substantially a light source. Take, for instance, the amount of tobacco smoke that can cloud up a room when the sun is shining in it, and it will be found to be a very small quantity, but, if the room be black as night and a hole be made in a shutter through which a small beam of sunlight enters and the minutest body of smoke be diffused in the room, there will be a "comet's tail" extending from the opening across the room where the sunbeam passes because it will be seen in blackness and that is the condition of our seeing comets' tails; in the darkness of night. Then we must remember how deep the space is which is occupied as a visible thickness in a comet's tail, say, 50,000 miles. We thus get an idea of how *free* of particles space must be *not* to shine with a luminosity equal to that of a comet's tail when we look off into the dark night irradiated by the intense solar beams.

Doubtless the simple view here given is complicated by many other actions, electric, etc. Comet's tails sometimes vary greatly and rapidly. We need not be surprised at this when boiling points are known to be critical; when, in other words, a few degrees increase in temperature may vaporize a substance

which would not otherwise have been vaporized. Furthermore, it is quite possible that the comet, in moving around the sun, entangles itself in the stream of material driven from the sun and varies in its effects in accordance with its being or not being in a solar streamer more or less dense for the time being, speaking relatively. It is easily conceivable that an assumed stratification of space may be a cause of variations of comet's tail brightness. Putting it more properly, it is conceivable that a comet may act as an indicator of the condition of space around the sun, the space in which the comet, for the time being, is moving. Even under the idea that there is volatile matter emitted from the sun which ordinarily would not be visible, let such matter strike into the nucleus of a comet and meet matter from the comet itself; it is easily seen that interactions, electrical or otherwise, or even physical collisions, may add to the light of a comet's tail.

The chief point, however, which I have endeavored to emphasize by the comparisons above made, is the excessive tenuity of the matter which would be sufficient to give rise to a brilliant appendage to a comet and the exceedingly small amount of volatile matter needed. This fact renders it possible that the comet may, in the lapse of many years, replenish itself in the depths of space and may account for the fact that at each return, even to close proximity to the sun, a tail is developed. Otherwise, since the matter of the tail certainly does not return to the comet, it would seem that the volatile matter would be distilled off and lost in a very few perihelion passages.

Elihu Thomson

ROBERT PARR WHITFIELD

Professor Robert Parr Whitfield died on April 6 at Troy, N. Y., in his eighty-second year.

Professor Whitfield's association with the progress of paleontological science in the United States has placed his name permanently among the pioneers of that science in this country. His work, however, has no antiquarian interest merely. From the first it was forcible, careful and convincing. Throughout the long period of his connection with the American Museum of Natural History he industriously contributed papers on invertebrate paleontology, to the publications of that institution, while his work on the surveys of Ohio, Wisconsin and New Jersey was persistently prosecuted, in reports of great value, distinguished always by keen morphological discrimination.

His work began with his employment on the New York State Survey, where he assisted Professor James Hall, who was then engaged in his studies of Paleozoic fossils. Professor Whitfield's assistance was at first in the nature of exact preparatory analyses of the copious material offered for examination, classification and description. About this time he produced the beautiful illustrations of graptolites which gave distinction and an unusual interest to the decades of the Canadian Survey, and his painfully minute study upon which superinduced a, fortunately, only momentary, danger to his eyesight. He continued his labors on the survey until 1877, and helped materially to give precision and a broad zoological basis of comparison to the reconstructions of the invertebrate life of the past, in the papers and volumes, written by Professor Hall, not only upon the paleontology of New York, but of western states as well. His studies of the internal loops of various genera of brachiopoda, his delineation of the muscular scars of lingula and his rearrangement of the crinoidal scheme of plates were all very helpful. Succeeding this came his admirable descriptive papers published in the geological reports of Wisconsin and Ohio. Then followed an exhaustive examination of the upper Devonian lamellibranchs, the results of which were embodied in the subsequent New York survey volumes on these shells.

When the great Hall collection of fossils came into the possession of the American Museum, Professor Whitfield was invited to take charge of this extraordinary cabinet, to

install, arrange and label it. It would have been impossible to have found any one so well qualified for this task; he seemed to recognize every specimen as it was unpacked and each one became the text of pleasant or exciting memories.

It was not long after Professor Whitfield's assumption of this important charge that the publication of the *Bulletin* of the American Museum was begun, and paleontological papers from his pen appeared upon its pages. It is quite unnecessary to review all of these; they consisted of descriptions of new species, genera, revisions, notes, emendations and figures of hitherto unfigured species, and original identifications and discussions. Perhaps the most important were his descriptions of the fossils of the Fort Cassin beds in Vermont, his admirable treatment of the subject of *Uphantaenia* and *Dictyophyton,* referring these problematic bodies to sponges, a position firmly established by later observations, his detection of a fossil scorpion in the Waterlime beds of New York, his papers on Cretaceous Syrian fossils, on fossil marine algæ, on the Cretaceous Rudistæ of Jamaica and his review of the anomalous genus *Barrettia* from the same island. He occasionally intercalated in these fossil studies a paper upon living forms, as his experimental observations upon *Lymnea megasoma,* a new sponge from Bermuda and a new coral from the Bahamas.

He completed during these years his great work on the fossils of the Cretaceous and Tertiary of New Jersey, a work achieved under very serious difficulties, and with most fragmentary and insufficient material. These memoirs were published by the U. S. Geological Survey. The genus *Whitfieldia,* a member of the meristelloid brachiopods, was named by Professor Davidson after him, and his name as a specific designation appears up and down the pages of paleontographical literature. Unostentatious, of a reserved, almost severe demeanor, animated by an intense love of his science, his life was passed peacefully and pleasantly, amid unruffled domestic relations, in unbroken association with the objects of his conscientious and unremitting study.

L. P. G.

CONFERENCE ON AGRICULTURAL NATURE-STUDY

THE conference on the teaching of agriculture in the common schools of Illinois was held from March 24 to 26, an enthusiastic session at the University of Illinois at Urbana. This was the first meeting of its kind in the United States, and educators from all over the state of Illinois and neighboring states took part in its sessions. Among those present were D. J. Crosby, U. S. Expert in Agricultural Educational Work, Washington, D. C., and representatives of railroads, members of agricultural faculties from neighboring states, members of the legislature, county superintendents, normal school faculties, farmers' institute officials, rural school directors, domestic science leaders, manual training leaders, practical farmers and land owners, technical men, college and university professors, state departments of public instruction.

The conference was inclined to move slowly along this new line of activity. It took, however, two or three steps that are destined to be very important in the educational work of the schools of the state. It was strongly urged that a course of study in agriculture be planned for the elementary schools of the state. A committee was appointed for this purpose consisting of Professor Fred. L. Charles, University of Illinois; County Superintendent McIntosh, Monticello, Illinois; Miss Alice J. Patterson, State Normal University, Normal, Ill.; Assistant State Superintendent, U. J. Hoffman, Springfield, Ill.

It was arranged that a second meeting of the conference be held next year in connection with the agricultural short course at the University of Illinois, when something over a thousand people of the state will be assembled to study agriculture in its various phases.

The following standing committee was ap-

pointed by the conference to continue the organization and work of the conference: Assistant State Superintendent U. J. Hoffman, chairman; Anna L. Barbre, county superintendent, Taylorville; C. H. Watts, county superintendent, Champaign; Hon. J. B. Burrows, Decatur; Mrs. Scott Durand, Lake Bluff; Alice Jean Patterson, Illinois State Normal University; Professor W. G. Bagley, University of Illinois; Professor Fred. L. Charles, University of Illinois.

The above-mentioned committee presented the following resolutions which were unanimously adopted by the conference:

Resolved:

1. That this conference request and authorize Professor Fred L. Charles to appoint a representative committee to serve with him as chairman in the preparation of a course of study in agricultural nature-study which may be suitable for the eight grades of the elementary schools of Illinois.

2. That we may request those who are responsible for the conduct of the agricultural short course, that provision be made for a second meeting of this conference during the next annual short course at the university.

3. That this conference appoint a committee of three to enter into communication with the Illinois Farmers' Institute, through its committee on Agricultural Education in the public schools, to bring to its attention the urgent necessity of furnishing to the teachers of the elementary schools of the state all possible aid in the organization and adaptation of agricultural materials suitable to the purposes of these schools, and, further, to request that they take such action as they deem necessary to secure at the next session of the legislature ample funds to equip the University of Illinois, through its college of agriculture and school of education, to carry on the following most essential lines of work: (1) Research in the organization and method of nature-study and agriculture in the elementary schools; (2) the training of specialists within this field; (3) the publication of abundant literature for the use of the public schools; (4) the maintenance of a correspondence bureau to meet the rapidly growing demands from the teachers and elementary school interests of the state; (5) the establishment and maintenance of a bureau for the preparation and distribution of equipment and materials essential to instruction in this subject; (6) the employment of thoroughly competent demonstration teachers who shall be sent out into the state to assist in the introduction of this study in the elementary schools; (7) such other means of advancing this study as may later appear to be desirable.

The committee of three to communicate with the Farmers' Institute Committee was as follows: Hon. Joseph Carter, Hon. J. B. Burrows, Dean Eugene Davenport.

One interesting result of the work of this conference was the plan to assemble at the university model rural school equipments in agriculture, domestic science, hygiene and public health, and in manual training. The university is very fortunate in already being provided with an equipment in manual training. This model outfit for rural schools was presented to the university by the Bradley Polytechnic Institute of Peoria, and is the product of a study by Professor C. S. Van Deusen.

A statement has gone out to the papers that manual training in the schools had received a set-back in the discussions of this conference. The statement was entirely erroneous, nothing to that effect was even suggested.

REPORT OF AN INVESTIGATION OF THE PHENOMENA CONNECTED WITH EUSAPIA PALLADINO

The undersigned had three sittings with the Italian medium Eusapia Palladino in the Physical Laboratory at Columbia University in January last. The object in view was to secure and report any evidence of the operation of hitherto unknown forces through her or in her presence.

Though the investigation may fairly be called patient and laborious, no convincing evidence whatever of such a phenomenon could be obtained. Many indications were obtained, however, that trickery was being practised on the sitters. These indications will be more fully stated by the individual investigators.

So far as these sittings afford data for judgment, the conclusion of the undersigned

is unfavorable to the view that any supernormal power in this case exists.

CHARLES L. DANA, M.D., *Professor of Nervous Diseases, Cornell University Medical College.*

WILLIAM HALLOCK, *Professor of Physics, Columbia.*

DICKINSON S. MILLER, *Professor of Philosophy, Columbia.*

FREDERICK PETERSON, M.D., *Professor of Psychiatry, College of Physicians and Surgeons, Columbia.*

WALTER B. PITKIN, *Lecturer on Philosophy, Columbia.*

AUGUSTUS TROWBRIDGE, *Professor of Physics, Princeton.*

EDMUND B. WILSON, *Professor of Biology, Columbia.*

ROBERT WILLIAMS WOOD, *Professor of Physics, Johns Hopkins.*

It has been said that Eusapia finds trickery more easy than the exercise of her supernormal power; that she consequently resorts to the former whenever the control by the sitters permits it; and that the only fair test is had when there is such control as makes trickery absolutely impossible. During a fourth sitting, at which the undersigned were present, something like this control was exercised; and while this was the case none of the so-called evidential phenomena took place.

C. L. Dana, W. Hallock, D. S. Miller, F. Peterson, W. B. Pitkin, E. B. Wilson.

We take this opportunity of making our acknowledgments to Professor Hallock for his courtesy in putting his private office and workshop at the disposal of the investigators; and to the members of the groups at large for giving their time to the sittings in the midst of professional duties, in especial to those who came from a distance. We wish to record our regret that, owing to circumstances beyond our control, the X-ray test, ingeniously devised by Professor Wood, could not be applied.

W. P. MONTAGUE,
W. B. PITKIN,
D. S. MILLER

I have been present at nine sittings with Eusapia and in an adjoining room at a tenth. Broadly speaking, her "phenomena," as observed in America and as reported before, fall into seven classes: (1) levitations of a table, (2) rappings, (3) touches, (4) breezes, (5) lights, (6) "materializations," (7) movements in and about the cabinet. With the lights I was not favored. Of all the other classes, I can say: (1) That conclusive and detailed evidence was gained as to the method by which typical specimens of them were repeatedly produced,[1] and (2) that when the medium was securely held they were not produced at all.

Statements of observations on essential points will, I trust, be published later. These include each of the classes named.

It may be asked, however, what we are to make of the results presented in the *Bulletin* of the *Institut Général Psychologique of Paris* and in the *Proceedings of the Society for Psychical Research*. Of these two documents it is, by common consent, the latter which presents the strongest body of evidence for Eusapia's supernormal power. The Paris committee had worked mainly to establish that the "phenomena" really occur and are not the mere hallucinations of the sitters. Of course they do occur; we must admit it. But the English committee try, by reporting in detail how the medium was held and watched, to give the reader evidence that the phenomena could not have been caused by trickery. The result is that we have the record of a long, hard and conscientious piece of labor. It is imposing. It seems at first to warrant the writers' unanimous "Yes, the thing is true." But read Richard Hodgson's comments on the case, written sixteen years ago when he was in consultation with Mr. W. S. Davis; or read Mr. W. S. Davis's article in the New York *Times* of October 17 last. Read one of these enough to grasp it; then attend one sitting; and the impressive effect of the English report has vanished. One finds himself able to point out on page after

[1] Accounts are presented in the article by Professor Jastrow in *Collier's Weekly* for May 14, 1910.

page how the writers were deceived. On page after page one finds them the victims of the old "substitution-trick." Examples of this will be given elsewhere. One can go through the report and write on the margin at almost every phenomenon (where the "control" is stated) by what hand or foot it was probably done. No substantial evidence remains.

Thanks are due to Messrs. W. S. Davis, J. L. Kellogg and J. W. Sargent, who have all had much experience, both of professional conjuring and of the investigation of mediums, and who gave their time and invaluable services at my last two sittings. Mr. J. F. Rinn, a merchant, who is a trained observer and an investigator of spiritualism, deserves special acknowledgments for his work as a watcher. DICKINSON S. MILLER

I agree substantially with the committee's report. My sittings with Paladino have failed to convince me that she possesses any unknown force. In fact, she has been detected in so much trickery that there is in my opinion an extremely high probability that all of the manifestations which I witnessed were produced by merely natural means. But I do not feel that the methods and conditions of our experiments were of such a kind as to warrant the rigorously scientific and finally conclusive verdict for which we had hoped, or even to justify quite the degree of emphasis expressed in the majority report.

It has long been known that Paladino resorted to trickery, and the claim has been made and will still be made that she finds it easier to perform fraudulently that which she can and sometimes does accomplish otherwise. The Cambridge exposure of 1895 proved that she used trickery, but did not put a stop to her scientific vogue. I had hoped, perhaps foolishly, that our investigation would be rather more than a repetition of something already accomplished. And it seemed plain that the policy to pursue was to insist upon conditions of control by mechanical means, which, instead of encouraging fraud by their looseness, should be so rigorous as absolutely to eliminate her well-known tricks of foot and hand substitution.

If this plan had had a fair trial, and no "phenomena" had resulted, our report might have given a permanent quietus to the Paladino cult. W. P. MONTAGUE

I sign the majority report, believing it correct as far as it extends. But it does not go far enough. It gains, I think, a certain fictitious importance through the absence of all those details about methods and results which are properly considered indispensable to any such statement made by scientists to scientists. Were those details here recorded, the difference between this report and the sort SCIENCE usually prints would instantly appear.

One may take either of two attitudes toward Eusapia and her like. Judge her by shrewd common sense, if you choose; then almost everybody will briefly pronounce her an egregious and unmitigated humbug, as I do when thus considering all that I have seen at seven of her seances. On the other hand, though, you may prefer to subject her phenomena to the strict scientific method; and now, having elected the intellectual game you are to play, you must observe its rules. If my understanding of the canons of induction is correct, the investigators sometimes unwittingly and sometimes unavoidably changed their point of view very often in the midst of their experiments with the result that their verdict, like my own, is based upon impressions and "human" probabilities. That these latter are very strong does not make the conclusions from them scientific. Perhaps it is not worth while trying to be scientific over such matters, but that is another issue. W. B. PITKIN

Professor Miller has asked me to add to the statement which I signed as a member of the committee, a personal report of the impressions made on me by the three sittings with Eusapia Palladino which I attended in January.

Judging from the earlier sittings which I attended on the invitation of Mr. Hereward Carrington, I should say that those held with the committee were fairly representative as

regards the class of phenomena which Palladino has attempted to produce in this country, though as regards quantity, rather than quality, they should be regarded as poor sittings.

I was particularly struck by an incident which occurred during the third sitting (January 22, 10:32 P.M.) which goes to show how very cautious one must be in accepting as evidential motions of objects apparently out of reach of the medium.

From 10:29 P.M. until 10:32 P.M. objects were moved in the cabinet behind E. P. while she was under the following conditions of control—feet tied together by a rope which prevented her from separating them by more than eight inches, in addition her ankles were held by one of the sitters who had taken up a position on the floor, each wrist tied to a wrist of her neighbor, on the right and left, by means of a rope which allowed her ten inches free motion in case she should elude the tactile control which her neighbors were endeavoring to keep. The light in the room at the time was that from a frosted electric bulb which I estimated to be giving about four candle power, placed about four feet from the medium's head.

It would seem that the objects moved in the cabinet were outside the range of free motion of her hands and feet and the motions seemed to be taking place under what might be called "test conditions." However, the shorthand report of this sitting shows that three of the sitters were convinced that the motions were caused by the medium knocking over objects in the cabinet with the back of her chair—I noted that so soon as her chair was moved openly a few moments afterwards more objects fell.

I mention this particular incident as I think it shows how difficult it is to obtain really "test conditions." Incidents of a similar character at other sittings I have attended, where at first sight the conditions of control seemed excellent, have rendered me extremely reluctant to base an opinion as to this remarkable woman on the very interesting reports of her numerous European sittings, but, so far as the evidence collected at the relatively uninteresting American series of sittings is concerned, I think it is decidedly unfavorable to Eusapia Palladino's pretensions.

AUGUSTUS TROWBRIDGE

After attending six sittings with Eusapia Palladino, I find myself in much the same position as at the start.

Many things have occurred which I find great difficulty in explaining by fraud while I have repeatedly seen trickery employed. I have succeeded in watching the manifestations within the cabinet throughout two entire evenings, the floor being illuminated with a feeble light which was thrown by means of a mirror through a crack between the bottom of the cabinet and the floor. The cabinet was of wood built into a doorway, so that it projected back into the adjoining room. My plan was to employ a powerful X-ray apparatus and a large fluorescent screen, so that a shadow picture of whatever was going on within the cabinet could be obtained in the back room at any instant without the knowledge of the medium.

The interior of the cabinet I viewed through a large hole cut in the top, reclining on a mattress placed on the top of an instrument case adjoining the doorway. The X-ray tube was placed within the instrument case and carefully muffled, the fluorescent screen three feet square was placed against the opposite wall of the cabinet, on the outside of course. This apparatus was never actually used, owing to the sudden termination of the sittings, but it was set up and thoroughly tested, and gave excellent satisfaction. I mention it as it may be of use to future investigators, for, if properly installed, it is proof against any fraud, as it can be used without the medium's knowledge.

From my position above the cabinet I saw that whenever anything in the cabinet was moved the curtain was pushed back, a black object reaching in from Palladino's back groping around and finally seizing the table. Those who believe in Eusapia's supernormal powers will say that this was the third arm.

I need not say that an X-ray picture of this third arm as seen on the fluorescent screen would be an interesting subject of study. It would not be difficult to so arrange the apparatus that the shadow of the medium's entire body could be obtained. The switch for operating the coil should be placed within reach of the observer on the top of the cabinet, so that the flood of rays need only be turned on when something is going on worth investigating. In this way no possible injury could result.

At the first sitting at which the illumination of the floor was tried Eusapia complained of this light, which appeared to be quite accidental. I accordingly constructed a grill of vertical strips of thin wood, painted black. The floor of the cabinet was covered with this. From her position in front Eusapia could not have seen the light on account of the grill, while the observer above, looking down directly between the strips, could see the illuminated floor without difficulty. The object of illuminating the floor was of course to obtain a luminous background against which moving objects could be seen. It proved to be a very effective way of investigating cabinet phenomena.

On two occasions the black object which appeared was pointed, on the third, when the table was seized it was blunt and rounded. Eusapia had pushed her chair back until her back was against the curtain, and I doubt if what I saw was the "third arm"! On the occasion when I held one of Eusapia's hands, nothing was disturbed in the cabinet, but some very fine levitations occurred, in a brilliant light, and I could not only see between the medium's knees and the legs of the table, but passed my other hand between them and her skirts. I felt very positive that the legs of the table were free from contact with any part of her person.

The proper system of investigation, in my opinion, is the one outlined. Whenever I saw anything going on in the cabinet, I sent an electric signal to seance room, so that particular pains could be taken by the persons holding her hands, to see whether the contact had been broken at the moment.

If the phenomena are genuine it can be proved by the X-rays, I think, *and in no other way.* Madam Palladino need have no fear of the X-ray test, if the thing seen in the cabinet is a supernormal third arm. If the sittings had not been suddenly terminated, I feel certain that at the next one we should have had a complete explanation of how the disturbance in the cabinet was created. I am quite ready at any time to aid Madam Palladino in establishing the genuineness of her supernormal powers by means of the X-rays.

R. W. Wood

THE CARNEGIE FOUNDATION

The following letter has been addressed to the trustees of the Carnegie Foundation for the Advancement of Teaching:

Columbia, Mo.,
March 9, 1910.

To the Board of Trustees of the Carnegie Foundation for the Advancement of Teaching.

Gentlemen: At a largely attended meeting of members of the faculties of the University of Missouri it was voted that the following communication be addressed to your board as a body, and to its individual members:

The purpose of the foundation of which you are the administrators, as set forth in the expressions of the founder and in subsequent official statements of the trustees, are "to serve the cause of higher education by advancing and dignifying the profession of the teacher in higher institutions of learning," especially with a view of rendering that profession attractive to increasing number of able men. Through the desire of many institutions to enjoy the benefits of the foundation, it has come to be also an important instrumentality for influencing and coordinating the educational policy of a considerable number of American universities and colleges. It is evident that these functions, of great potential usefulness, can in the long run be successfully performed only if the management of the foundation retains the confidence and sympathy of university officials and of the general body of university teachers. Though the foundation may do something to increase the material comfort in old age of some

members of the teaching profession, it can not accomplish its announced primary purpose unless its activities are such as, in the opinion of the majority of university teachers, actually tend to advance and dignify their profession. And it can not long retain the beneficial influence which it may properly exercise over the policies of institutions, unless their faculties and governing boards continue to believe that the foundation will fulfill the promises implied in its rules.

Certain recent acts of the foundation appear to us to be not only inequitable in themselves but also to be likely to destroy the confidence of university teachers and university boards in the stability of the foundation's policy, in the trustworthiness of its announcements, and in the general tendency of its work to render the profession more attractive to young men of independent spirit and high ability. While we do not feel called upon to express any opinion concerning the intrinsic desirability of a general and unqualified system of length-of-service pensions, we consider the abrupt abolition of such a system, without notice, after individuals and institutions have for four years been basing their acts upon the foundation's announcement that it would grant such pensions, to be unfair to those directly affected and provocative of indignation in nearly all teachers not directly affected. We, therefore, respectfully request that your board, as early as may be convenient, reconsider its action upon this matter. We believe, also, that further legislation is desirable, with a view to reassuring the academic public against the anticipation of other sudden and radical changes of the foundation's policy, and with a view to promoting a better and more sympathetic understanding between the management of the foundation and the general body of teachers.

While we do not desire to suggest the details of the legislation to be adopted, we are of the opinion that some such measures as the following would make for the advancement of the teaching profession, and therefore for the realization of the purposes of the foundation:

1. The adoption by your board of such supplementary legislation as shall effectually safeguard the interests of those who have, during the past four years, been influenced in the conduct of their affairs by expectations aroused by the old service-pension rule.

2. The adoption of a new rule, whereby no essential changes may be made in any of the fundamental rules of the foundation without several years' notice, duly promulgated to all of the institutions upon the accepted list.

3. The inclusion in the membership of the board of trustees of representatives of the teaching branch of the profession.

All of which is submitted to your favorable consideration.

C. STUART GAGER,
W. I. DAUMFORD,
H. B. SHAW,
Committee

SCIENTIFIC NOTES AND NEWS

PROFESSOR SVANTE ARRHENIUS, of Stockholm, has been appointed Silliman lecturer at Yale University.

DR. GEORGE E. HALE, director of the Mount Wilson Solar Observatory, has been elected an honorary member of the Royal Institution, London.

CAMBRIDGE UNIVERSITY will confer honorary degrees this term on Sir Oliver Lodge, F.R.S., principal of the University of Birmingham, and Professor W. H. Perkin, F.R.S., professor of organic chemistry in the Victoria University of Manchester.

AT the meeting of the Royal Society on May 5 the following candidates for fellowship were elected into the society: Mr. J. Barcroft, Professor G. C. Bourne, Professor A. P. Coleman, Dr. F. A. Dixey, Dr. L. N. G. Filon, Mr. A. Fowler, Dr. A. E. Garrod, Mr. G. H. Hardy, Dr. J. A. Harker, Professor J. T. Hewitt, Professor B. Hopkinson, Dr. A. Lapworth, Lieutenant-Colonel Sir W. B. Leishman, Mr. H. G. Plimmer and Mr. F. Soddy.

AT a meeting of the American Academy of Arts and Sciences, held on May 11, it was voted to award the Rumford premium to Charles Gordon Curtis "for his improvements in the utilization of heat as work in the steam-turbine."

DR. F. L. CHASE has been appointed acting director of the Yale Observatory.

PROFESSOR FREDERIC P. GORHAM, of the biological department of Brown University, has been appointed by the commissioners of shell fisheries of the state of Rhode Island to make a study of the distribution of the sewage in

Narragansett Bay in relation to the oyster beds.

Mr. P. H. Cowell, F.R.S., chief assistant in the Royal Observatory, Greenwich, has been appointed superintendent of the Nautical Almanac, in succession to Dr. A. M. W. Downing, who has retired.

Professor Fitzgerald has resigned the chair of engineering in Belfast University.

Dr. Harvey W. Wiley, chief of the Bureau of Chemistry, U. S. Department of Agriculture, has been elected president of the American Therapeutical Society for the coming year. The next meeting of the society will be held in Boston in May, 1911, under the auspices of the Harvard Medical School.

The Pennsylvania Chapter of the Society of the Sigma Xi has elected Professor I. J. Schwatt president, and Professor Wm. Easby, vice-president for the year 1910–11.

Mr. J. B. Tyrrell has been elected president of the Canadian Institute.

Mr. Alban Stewart, of the botanical staff of the New Hampshire College, has spent more than a year on the Galapagos Islands, making botanical notes and collections, which he has since worked up for publication at the Gray Herbarium of Harvard University, under the direction of Dr. B. L. Robinson.

Dr. Louis A. Bauer gave an address, under the auspices of the Joseph Leidy Scientific Society, "On the Cruise of the *Carnegie*," on May 10 before the students of Swarthmore College.

Mr. Douglas Mawson, professor of geology at the University of Sydney, is passing through the United States on his way to Australia.

It will be remembered that Mr. Henry Wilde, F.R.S., D.C.L., D.Sc., who had already founded at Oxford the Wilde readership in mental philosophy, the John Locke scholarship, and the Wilde lectureship in natural and comparative religion, established recently an annual lecture on astronomy and terrestrial magnetism, to be called the Halley lecture, "in honor and memory of Edmund Halley, some time Savilian professor of geometry in the university and astronomer royal, in connection with his important contributions to cometary astronomy and to our knowledge of the magnetism of the earth." Dr. Wilde delivered the first lecture on May 10, the title chosen by him being "On Celestial Ejectamenta."

It is announced that the erection of a laboratory for research in chemistry at Harvard University to be dedicated to the memory of Dr. Wolcott Gibbs is now assured. The small residue required has been underwritten by a friend. The site of the laboratory will probably be near the University Museum.

Dr. Noah Knowles Davis, professor emeritus of philosophy in the University of Virginia, has died at the age of eighty years.

Sir William Huggins, eminent for his contributions to astrophysics, past president of the Royal Society and of the British Association for the Advancement of Science, died on May 12, at the age of eighty-six years.

By action of the trustees of the Missouri Botanical Garden, five research fellowships in the Henry Shaw School of Botany have been established, each carrying an allowance of $500 per year. In memory of the late president of the board of trustees of the garden, who had held that office from the organization of the board until his death this spring, these are to be known as the Rufus J. Lackland Research Fellowships.

Reuter's agent at Georgetown, British Guiana, says that Sir Francis Lovell, dean of the London School of Tropical Medicine, has concluded his tour in the West Indies. His appeal for subsidies for the school from the various governments has been successful, useful sums being promised from all the British possessions. Barbadoes has promised £50 a year; the Windward Isles £50; the Leeward Isles £100; Jamaica £100, and Trinidad £100, and there is every likelihood that British Guiana will undertake to give a contribution.

In a report of the committee appointed by Provost Harrison, of the University of Pennsylvania, to consider plans for the future

operation of the Phipps Institute, it was recommended that its future policy embrace three fields of activity, and be reported by three correspondent departments—the laboratory, clinical and sociological departments. Work of the laboratory will be devoted principally to the discovery or formulation of some specific remedy for the treatment of tuberculosis. The program outlined for the clinical and sociological departments is divided into four parts: (1) The clinical and social work in allotted districts; (2) social research; (3) general educational work; (4) the stimulating of the public to action.

CONGRESSMAN MANN has introduced two health bills in the House of Representatives. One is a bill to enlarge the Public Health and Marine Hospital Service, changing the name to "The Public Health Service," while continuing it under the Department of the Treasury. The bill creates a division of water supply, to investigate the pollution of streams, and confers authority to investigate tuberculosis, typhoid fever and other diseases. The other is a bill embodying the original suggestions of the Committee of One Hundred before Senator Owen's plan for a department was presented. This makes, at least, six important public health bills now before congress, the other four being the Owen bill (S 6049) in the senate, the same bill in the house introduced independently by Congressmen Creager and Hanna, and a modification of the Owen bill introduced by Congressman Simmons.

AT the decennial convention for the Revision of the Pharmacopœia, held in Washington, D. C., on May 10, Dr. H. W. Wiley was elected president and Dr. Joseph P. Remington was made chairman of the revision committee of fifty which will be divided into fifteen sections to which are assigned specific subjects. The next meeting of the committee will be in 1920, but the work of revision is already under way and will be pushed as rapidly as possible. The delegates to this convention are accredited representatives of incorporated medical and pharmaceutical associations and colleges and of such other scientific societies and federal officials as are specifically named in the constitution, for example, the American Chemical Society and the surgeon-generals of the Army, the Navy and the U. S. Marine-Hospital Service. By amendment to the constitution at the last meeting the following additions were made to the list of officials and organizations authorized to appoint delegates: the Secretary of Agriculture, the Secretary of Commerce and Labor, the Association of Official Agricultural Chemists, the Association of State and National Food and Dairy Departments, the Wholesale Druggists Association and the National Dental Association.

THE School of American Archeology will continue during the year 1910 the work of exploration and excavation of ancient ruins with collateral ethnological and historical work in New Mexico, Utah, Arizona and Central America. The season for work in the southwestern part of the United States is from June 1 to November 1; in Central America it is from December 1 to May 1. Properly qualified persons will be admitted to the field expeditions of the school or to undertake research work under its direction in Santa Fé or elsewhere, on satisfying the staff of their ability for original investigation. Those who desire to undertake such work should write the director, Mr. Edgar L. Hewett, stating his or her wishes, giving such information as to qualification as would naturally be needed and stating when and for what length of time they desire to take up the work.

DURING the week of May 30–June 4 a party of students from the college of agriculture of the University of Wisconsin, under the direction of several members of the faculty, will inspect some of the fine farms, creameries and farm product manufactories of the southeastern part of the state. This form of instruction has been applied to students of animal husbandry in previous years, but has never before been given to students of agriculture in general.

UNIVERSITY AND EDUCATIONAL NEWS

It is reported that Yale University will appropriate from $60,000 to $80,000 a year for the increase of salaries of professors.

Wesleyan University has been admitted to the Carnegie Foundation for the Advancement of Teaching.

Dr. Robert Kennedy Duncan, professor of industrial chemistry at the University of Kansas, has accepted a call to the University of Pittsburgh.

Charles H. Shattuck, Ph.D. (Chicago), has been appointed professor of forestry in the University of Idaho.

Dr. James F. Abbott has been promoted to a professorship of zoology in George Washington University.

At Cornell University promotions to full professorships have been made as follows: J. I. Hutchinson and Virgil Snyder, in mathematics; A. W. Browne, in chemistry; E. M. Chamot, in sanitary chemistry; E. H. Wood, in engineering, and H. D. Hess, in machine design.

Mr. Nathan C. Grimes, instructor at the University of Wisconsin, has been appointed professor of mathematics in the University of Arizona.

At Stanford University, Dr. E. C. Dickson has been appointed assistant professor of pathology and Mr. Thomas B. Hine, acting instructor in chemistry.

Miss Annie Louise MacLeod, of Nova Scotia, has been appointed resident research fellow in chemistry at Bryn Mawr College.

At Haverford College, Professor A. H. Wilson, of the Alabama Polytechnic Institute, has been appointed associate professor of mathematics, as successor to Professor W. H. Jackson, who returns to England.

Clinton R. Stauffer, Ph.D., instructor in geology at Western Reserve University, has been appointed assistant professor of geology in the School of Mining (Queen's University) at Kingston, Ontario.

Dr. E. J. Goddard, Linnean Macleay fellow in zoology, Sydney, has been appointed by the council of Stellenbosch College, South Africa, to the chair of zoology and geology in succession to Professor R. Broom.

DISCUSSION AND CORRESPONDENCE

THE LENGTH OF SERVICE PENSIONS OF THE CARNEGIE FOUNDATION

To the Editor of Science: So many errors have been put forth under the protection of anonymity, and this is deservedly in such disrepute, that the only excuse I can give for not signing my name to this note is the self-evident one that some of those to whom I refer might thereby be recognized.

I have read with interest the rather caustic criticisms on the change of the policy of the Carnegie Foundation with reference to voluntary retirement after twenty-five years of service, and must confess that some of these criticisms read to me, doubtless wrongly, as though they proceeded by some process of indirect inspiration from persons who had hoped to give up their teaching duties and that this disappointed hope had rendered them somewhat acid.

As a comparatively young man (38) whose twenty-five years of teaching and scientific work will not end for nine years more, may I give my opinion on the new ruling?

I regard the Carnegie Foundation as one of the most signally useful methods that could be devised to elevate the dignity and honor of the profession of teaching. I do not see how any teacher can fail to feel more assured as to his own future and that of his family as a result of these rather generous provisions. Very few of us save anything and it certainly gives one a sense of greater ease and freedom from worry to know that when those days come when one must perforce feel that advancing age renders impossible the old-time efficiency, provision has been made for the passing of the closing years of life in dignity and honorable independence; would that the provisions of the foundation could be extended to every teacher in state, church, city and country schools.

Why should any one *wish* to retire after

twenty-five years of service? If disabled or incapacitated the foundation makes such retirement a possibility, and doubtless a welcome one to some few to whom fate has been or may be unkind. But the average professor after twenty-five years of service is at his best as regards maturity, solid productive ability, and influence over youth through the poise and weight given by years and experience. Personally, I should hate to retire after twenty-five years of work, though I admit that the power thus to enjoy one's *otium cum dignitate* as a well-earned reward, and the possibility of doing just the work one likes best without hampering scholastic duties appeals strongly to universal human nature, and confess that it might conceivably appeal very strongly to me.

I know of several men, personally in one or two cases and by hearsay in other cases, who had hoped to take advantage of the twenty-five-year provision within a few years. As far as I know, they are all doing good and valuable work, are all in good health, are under fifty-five—in one case by a considerable margin—and I do not believe that they are worked too hard. All are thoroughly honorable, upright men, and are honest with themselves in believing that they are justified in trying to take advantage of this provision. Personally, and perhaps wrongly, I feel that their retirement at this time would be to some extent a misuse of the foundation, and amounts almost to a desertion of their post of duty. Were we in a Utopia where all, business men, mechanics, professors and scientists, could rest and play after reaching fifty, we as a world might be much happier. By "rest and play" I mean working hard at the work we love best. Till we reach that Atlantis, however, our thanks for the blessing of work as long as we can work.

Doubtless the men to whom I have referred will continue their productive work, though one had no definite plans other than retirement to his farm. Now I may not know all the circumstances which prompted these men to seek retirement after twenty-five years of service, but I can not feel that the purposes of the foundation would have been strictly adhered to should this be granted them. I can not feel that the withdrawal of the privilege of retirement after twenty-five years works any injustice; the error came in lack of foresight in announcing this provision at the start. We need vigorous, young, enthusiastic men, but the more respected, well-poised, experienced men between fifty and sixty-five *plus* we can keep on our faculties, the better for our institutions. Z.

SCIENTIFIC BOOKS

The Gulick Hygiene Series. By LUTHER HALSEY GULICK. Book One: *Good Health.* By FRANCES GULICK JEWETT. List price 40 c. Book Two: *Emergencies.* By CHARLOTTE VETTER GULICK. List price 40 c. Book Three: *Town and City.* By FRANCES GULICK JEWETT. List price 50 c. Book Four: *The Body at Work.* By FRANCES GULICK JEWETT. List price 50 c. Book Five: *Control of Body and Mind.* By FRANCES GULICK JEWETT. List price 50 c. Boston, Ginn and Co.

The editor states the objects and general plan of these books as follows:

> The objects of this series of books on hygiene is to teach the fundamental facts of health in such a way that the teaching shall result in the formation of health habits by the children. . . . In order to maintain the interest and avoid the deadening effect of the annual review of identical subjects, I have endeavored to supply each year some distinctive and separate line of thought in hygienic directions. . . . The style of the series is rather that of the story than that of the text-book.
>
> In four respects we have attempted in this series to do what so far as we know, has not been attempted before. (1) We have endeavored to present to children a series of texts in which the central theme shall be hygiene. The current text books treat of physiology and anatomy primarily. . . . (2) It is the purpose of this series to treat each subject in a purely scientific as distinguished from a philosophical manner. . . . (3) We have presented a new point of view in each volume. . . . (4) These little volumes have been prepared with the same kind of utilization of original works as if they had been intended for adult scientific workers.

The volume entitled "Good Health" was written for the fourth grade. In this a general view is taken of the subject. Scarcely any anatomy and relatively little physiology are given, the main contents of the book consisting of concrete and interesting facts relating to pure air, tobacco, cleanliness, sleeping, eye-sight, alcohol, hearing, finger nails, hair, care of nose and teeth, and eating.

The second volume in the series, "Emergencies," approaches the subject of the formation of habits from the standpoint of the emergencies which come to children. The skin is discussed . . . from that [standpoint] of blisters and burns. The habits that it is desirable for children to form with reference to conduct during emergencies form the subject matter of the year. . . .

The volume "Town and City," which is prepared for the sixth year of school life, presents the subject of hygiene from the standpoint of the community, and habits of action which have a social bearing are discussed; . . . the results of overcrowding, clean streets, garbage, ashes and refuse, parks, playgrounds, public baths, water supply, preventable diseases, food inspection, epidemics, vaccination, tuberculosis, city health, alcohol, microbes and disease. These are all topics in which individual action is involved. In all of them the relation and special emphasis are with reference to the state. The book is thus made an agency for the formation of habits having a community bearing.

The fourth volume, "The Body at Work," which is intended for the seventh grade, covers somewhat in detail the subjects ordinarily covered in the standard physiologies, but emphasis is laid on the training of the body for efficiency. Thus much is said concerning the importance of good posture and how to secure it; how one trains the muscles of the body that they may be efficient, enduring and strong; the nature and character of useful exercise; how digestion is most efficiently carried on. . . .

The closing volume of the series relates directly to the establishment of habits themselves— "Control of Mind and Body." In this book is discussed with some detail how habits are formed, not so much as a theory but as an experience; how habits are broken, fatigue, the wholesome development of the brain and spinal cord, the freedom which well-ordered habits give to the person who has them, the nerve endings, their care, etc. The whole purpose of the book is to give the individual that information which is related to the establishment of wholesome habits, particularly wholesome habits which shall be effective in the control of conduct.

A careful examination of these books justifies the following characterization:

1. They are written in a clear, readable style that is attractive and likely to be interesting to children.

2. They represent a serial story rather than a series of elementary and more advanced presentations of the same material. Each book is a new book on a new subject (as compared to the preceding book).

3. The facts presented are drawn largely from the results of accepted scientific investigations. The authors have made painstaking use of recent authoritative, scientific literature (for example note the discussion of the structure and physiology of the brain, and Cannon's experiments on intestinal movements).

4. The general motive, as indicated in the prefaces, is of a high order. The authors aim at human efficiency. The acquisition and conservation of health is regarded as an indispensable means to that higher end.

5. These qualities combine to make this an exceptional series of books, appearing in marked contrast with the conventional school text with its stereotyped style, its repetitions of text and illustration, its philosophical origin and consequent scientific inaccuracy, its limited scope, and its narrow ideal.

Several minor criticisms may be advanced as follows:

1. Book one, "Good Health," would be more complete if it contained some reference to the care of the excretions.

The system of ventilation shown diagrammatically on page 28 is an approved plan. It is backed by some of our best authorities. It is only fair to say, however, that such systems rarely work.

2. Some of the treatment given in book two, "Emergencies," is too advanced for children of the fifth grade. It contains a good deal of treatment that should be administered only by persons of some maturity.

Poisonous antiseptics should not be trusted to irresponsible children. The chapters on foreign bodies in the eye, on bandaging, and on poisons and their treatment, contain methods of treatment which would be unsafe in the hands of children.

3. One would expect a discussion of the "typhoid fly" in book three, "Town and City." Investigations of the last few years indicate that the fly is a most important factor in community hygiene.

The investigations of Meylan on smoking which have appeared since this book was written seem to throw considerable doubt upon the method and conclusions of Dr. Seaver's work, which is so liberally quoted in this book. Many of our discussions of the injurious effects of tobacco and alcohol need the careful and painstaking supervision of a trained investigator. It is easy to make serious mistakes in drawing conclusions from experiments and observations which are not properly checked with controls, or in comparing effects when the causes are complex and diverse, and therefore not productive of effects that will permit legitimate comparisons.

4. Book four, "The Body at Work," emphasizes good posture. There can be no doubt concerning the evils that accompany marked spinal curvature or a marked flattening of the chest with a great rounding of the shoulders. But so far as I know, we have arrived at our conclusions relative to cause and effect in these conditions philosophically and not scientifically. In addition I must admit, no matter how it offends my esthetic taste, that I have seen very few perfectly straight backs and shoulders. Most men have a stoop, and nearly all of us show a spinal deviation.

It would appear on pages 29 and 30 that the cuts there given represent either smooth muscle fibers, or nucleated forms of lower animals. They are not the human striated variety which is there under discussion.

Page 31. The soleus and gastrocnemius muscles seem to have exchanged names—a very slight error and of no consequence.

THOMAS A. STOREY.
COLLEGE OF THE CITY OF NEW YORK

Agricultural Bacteriology. By Professor H. W. CONN, Wesleyan University.

The second edition of Conn's "Agricultural Bacteriology" has been materially reduced in volume and has been brought more within the compass of a text suitable to the needs of students in agricultural colleges. It rightly emphasizes the great importance of microbes to fermentative activities, as this type of organisms is of much moment in agricultural processes, both favorable and unfavorable.

While covering the ground on the whole in a thorough manner, the volume is marred, however, by a certain looseness of statement in some of its chapters that is a serious defect in a classroom text, and the book contains altogether too many typographical and textual errors for a second edition.

To cite a few: "Fermentation and decay (p. 26) are defined as progressive chemical changes taking place under the influence of organic substances (evidently organized substances is intended), which are present in small quantity in the fermenting mass."

Decay and putrefaction are characterized as decomposition of proteid matter, the distinction being that decay occurs in the presence of oxygen, while putrefaction takes place in its absence. It is, of course, well recognized that decay of carbonaceous matter occurs, and that meat and other proteids may also putrefy in contact with the air.

The nitrates in the soil are stated (p. 47) as ranging from 0.1–0.2 per cent. This figure accords more nearly with the total nitrogen content of the soil. "Nitrites are changed to nitrates by the addition of another atom of nitrogen" (p. 57), meaning, of course, oxygen.

Speaking of the *Azotobacter* type (p. 94) they are regarded as more vigorous than the aerobic type (*Clostridium*), meaning *an*aerobic. The bacteroids of legumes are repeatedly referred to (p. 99) as bacterioids. The bacteria concerned in manure production are all regarded as putrefying organisms (p. 109), while, of course, it is well recognized that

many of the organisms present in manure are not associated with the production of malodorous compounds.

Reference is made (p. 145) to *Bacterium acidi lactici* in some cases and then again to *Bacterium lactis acidi,* when evidently the same organism is meant. This is apt to confuse not only the beginner, but even the more advanced student.

Numerous typographical errors as misspelled words, "dropped" lines, etc., occur, but these are not so serious in a way, as they can readily be recognized, but textual errors as noted above are less easily perceived by the student.

Science should teach a student to be exact and definite, but when texts are placed before him that contain so many slips of the pen, it sets a standard that makes for inferior work.

H. L. Russell

SPECIAL ARTICLES

NOTE ON THE CHROMOSOMES OF NEZARA. A CORRECTION AND ADDITION

In my preceding accounts of the chromosomes in *Nezara hilaris* (1905–06) I described the idiochromosomes as being of equal size and failed to recognize a dimorphism of the spermatid-nuclei. I have recently discovered that this was an error; and it is one that I wish to correct in advance of a more detailed description because *Nezara* now stands as the original representative of that type of insects in which neither a dimorphism of the spermatozoa nor a quantitative difference of chromatin between the sexes can be seen.

That type was first based on the single case of *Nezara hilaris,* but I afterwards added to it the lygæid species *Oncopeltus fasciatus* on the strength of Montgomery's earlier observations on the male and my own unpublished ones on both sexes. I was led to reexamine *Nezara hilaris* because of the discovery that in the closely allied southern species *N. viridula* there is a typical and very unequal pair of idiochromosomes, which show the usual relation to sex. The reexamination, in comparison with *N. viridula,* proves that in my earlier account the idiochromosome pair was incorrectly identified, and that in *N. hilaris* there is in fact a slightly unequal pair of idiochromosomes. This is, however, not the smallest pair (which is common to the two species) as both Montgomery and I were led to believe from the size-relations seen in other forms, but one of the largest; and in the second division it does not lie in the outer ring, as the small one does (a very exceptional position for the idiochromosome pair, as I pointed out) but occupies the typical position at the center of the group. The inequality of this pair in *N. hilaris* may readily be overlooked, since it is but slightly marked—far less than in *N. viridula,* and perhaps even a little less than in *Mineus,* as heretofore described. Moreover, both idiochromosomes are more elongated than the other chromosomes and often of nearly the same diameter, but differ in length. In polar views, therefore, the inequality often can not be made out, though in side views it constantly appears. My former figure of such a view actually shows an inequality of this pair, but insufficiently, the smaller member being represented a little too long and thick. The inequality is often more marked than in the particular specimen there figured.

Nezara can, therefore, no longer stand as a representative of the "third type" recognized in my paper of 1906, and *Oncopeltus* must probably take its place. I say "probably" because the case of *Nezara* shows how readily a dimorphism of the spermatozoa may escape detection when only a slight size-difference between the idiochromosomes exists. Renewed studies upon *Oncopeltus* (a very favorable object) shows that a slight inequality of the idiochromosomes may in fact often be seen at every stage of the spermatogenesis, from the pre-synaptic period onward. Quite as often, however, they appear equal, and the size-variation appears to lie within the range of variability in the other chromosome-pairs. A final decision in regard to this species is reserved for a future more detailed account.

A second point of interest, formerly overlooked, is the existence in the second division

of both species of *Nezara* of a quadripartite chromosome, composed of two somewhat unequal components and having exactly the form of a butterfly with wide-spread wings. This element, always lying in the outer ring and in constant position with respect to the spindle-axis, divides equally into two double elements. Each spermatid-nucleus thus receives six single chromosomes (including one idiochromosome) and one double element; though the duality of the latter is often obscured in the later anaphases. This phenomenon may indicate that a change in the chromosome-number is in progress, the double element representing either the initial stages in the separation of one of the "autosomes" into two (as appears to have occurred in case of the X-chromosome of *Syromastes, Fitchia,* etc.) or the final stage of a fusion of two into one.

EDMUND B. WILSON

THE STRUCTURAL CHARACTERISTICS AND RELATIONS OF THE APODAL FISHES[1]

THE characteristics and relations of the Apodals (Apodes) have been involved in much uncertainty even to the present hour. Nevertheless, no order appears to be really more trenchantly differentiated when a sufficient number of skeletons is at hand. Their chief characteristics of ordinal value may be given as follows:

Order Apodes

The order of eels or apodals is composed of fishes with a skull specialized especially by its extension forwards and the coalescence of the ethmoid, vomer (and premaxillaries?) into one piece which projects and is clamped laterally and more or less backwards by the maxillaries, the fusion with the vomer (?) or loss of the premaxillaries, the slight development of the palatal and pterygoid systems, the junction of the parietal bones, the presence of a chain of suborbital bones, the single cotyloid condyle for the articulation of the vertebral column, the freedom and reduced development of the shoulder girdle (and in some the complete loss), the single coraco-scapular plate in which are ossified the hypercoracoid and hypocoracoid, the mesocoracoid being lost, the brain of the ordinary teleost type but with secondary olfactory lobes in front of the principal ones, the great development of the branchiostegal apparatus, and the development of a pneumatic duct between the air-bladder and alimentary canal, and the loss or abdominal position of the ventral fins. The species propagate in the sea and pass through a peculiar stage known as the *Leptocephalus* or *Atopichthys* form, a ribbon-like translucent condition from which develops a later eel-like stage.

All the known species have the familiar eel-like form in varying degrees, some being much stouter and others excessively elongated, but the form is not an ordinal character, although in this case to a large extent coordinated with such characters. The absence of ventrals which gave name to the order (Apodes) is falsified by extinct representatives of the family Anguillavidæ, although justified by all the living species.

Inasmuch as much difference of opinion has prevailed respecting the homologies of the supraoral dentigerous bones, and as silence respecting them might be interpreted as the result of ignorance or undue disregard of others, some explanation seems to be called for here. By many of the old anatomists, the upper lateral dentigerous bones were considered to be palatines, but that view, for the most part, has been long abandoned. Recent high authorities, however, have regarded the bones in question as not homologous for the Murænids compared with the rest of the Apodals. While the upper bones of the Anguillids and other platyschistous eels have been admitted to be maxillaries, the lateral dentigerous bones of the Murænids have been homologized with the palatines or pterygoids. In other words, according to one author, the Murænids have the "maxillaries absent, replaced by the palatopterygoid, the mouth bordered by the latter and the ethmo-vomer," according to another, by "the toothed ethmo-vomer and pterygoids." Such an interpretation implies that the dentigerous bones, so much

[1] Abstract of a communication to the National Academy of Sciences, April 21, 1910.

alike and so highly specialized, connected, too, in such an unusual way with the cranium, have developed from two extremely different sources; that (1) the usual dentigerous bones have retained in the platyschistous eels, the functions performed in other fishes but under a highly specialized form, while (2) they have been lost in the engyschistous eels and bones (palatopterygoid), which had been much reduced or atrophied in the others, have been highly developed in the same manner but at the expense of the dentigerous bones of the typical eels. No reason has been assigned for such interpretations but it is probable that the posterior connection with the cranium of the dentigerous bones of the Murænids was one cause. We are thus forced into one or other of the two forks of a dilemma: which is the more probable, (1) that bones of two very distinct and disconnected arches have been inversely developed at the expense of each other in a like highly specialized manner, or (2) that the vomer-ethmoid has projected in one type (Colocephals) more than in the others (Euchelycephals)? The latter alternative has been preferred by the present author.

As to the premaxillaries, they have been considered to have been lost by recent ichthyologists, but it is at least possible (or even probable) that they have been consolidated with the ethmo-vomer, as Peters and Jacoby contended.

The order, as now limited, is represented by two suborders, (1) the Enchelycephals, including most of the species, and (2) the Colocephales, including (so far as known) only the Muræenids. The only near relations of the apodals are the Carencheli, known only by a single species, which is distinguished by the distinct premaxillaries, free nasals, etc.

The Lyomeri, which have been generally associated with the apodals, are extremely distant and *contrast* with them by the absence of most of the characters distinctive of the order.

Theo. Gill

THE PROPER RESTRICTION OF EUCYNOPOTAMUS

Some time ago I proposed the name *Evermannella* to replace *Odontostomus,* as the latter was found to be preoccupied in mollusca. Since then, Dr. C. H. Eigenmann, overlooking my use of this name, again proposed *Evermannella* as a new genus of Characinæ, with *Cynopotamus biserialis* Garman as its type. Subsequently I renamed Dr. Eigenmann's genus *Eucynopotamus,* a fact he seems to have entirely neglected, as his later proposal of *Evermannolus* shows. Thus *Evermannolus* must be considered an exact synonym of *Eucynopotamus,* embracing the single species *E. biserialis.* The wrongly identified genus *Eucynopotamus* of Eigenmann may now be known as Galeocharax gen. nom. nov. (type *Cynopotamus gulo* Cope), to embrace the species *G. magdalenæ, G. humeralis, G. gulo* and *G. knerii.*

Henry W. Fowler

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THE AMERICAN PHYTOPATHOLOGICAL SOCIETY. II

The Mildew of Ginseng caused by Phytophthora Cactorum (Leb. & Cohn) Schroeter: Professor H. H. Whetzel, Cornell University. (Read by Mr. V. B. Stewart.)

The mildew has long been known to the ginseng growers of Japan. It is known as "Koshi-ore," meaning a "bending-at-the-loins," from the characteristic drooping of the leaflets at the end of the affected petiole.

The relation of *Phytophthora cactorum* to the disease was first discovered by Hori in 1904 as pointed out by Van Hook. He demonstrated the constant association of this well-known Phycomycete with the lesions on the ginseng. Van Hook discovered this disease in Ohio and New York in May, 1905. He reports the constant abundance of oospores of *P. cactorum* in the diseased stems. So far as can be determined from the literature on the subject, no inoculation experiments have even been made to definitely establish the causal relation of this parasite to this disease.

The writer has observed this disease on an occasional plant in ginseng gardens since 1906. An epidemic of it appeared in a large ginseng plantation in New York State in 1909, causing a loss of more than 20 per cent. in some beds. Microscopical examination of a large number of diseased plants showed the *Phytophthora* always present in great abundance.

A careful study was made of the morphology of the parasite and its relation to the host tissues. These studies showed much the same conditions as those reported by Hartig for this fungus on forest seedlings.

A series of careful inoculation experiments were made as follows: (*a*) with conidia from diseased plants to healthy ones, (*b*) with motile swarm spores in water to healthy plants, (*c*) with mycelium from pure cultures of the fungus to healthy plants.

In every case there was prompt infection, with the resulting lesions characteristic of the disease. Microscopical examination of the diseased portions showed the conidia and mycelium of *P. cactorum* in abundance.

Pure cultures of the fungus were obtained by peeling back the epidermis on diseased stems and transferring bits of diseased tissue to sterilized bean pods. Oospores are produced abundantly in cultures. The isolation of this fungus in pure culture has not heretofore been accomplished so far as the writer knows. It is therefore the third species of the genus *Phytophthora* to be brought under cultivation.

On the Relationship of certain Bacterial Soft-rots of Vegetables: Professor W. J. MORSE, Maine Agricultural Experiment Station, and Dr. H. A. HARDING, New York Agricultural Experiment Station.

The organisms studied include several named species of soft-rot bacteria, in addition to nearly forty other strains isolated during the progress of the investigation. They represent pathogens from various cultivated vegetables, and one each from the iris and calla lily, obtained from widely separated sections of Europe and the United States.

The data were accumulated in two different laboratories, extending over a period of several years, and the more important determinations were checked by four different workers. Some 12,000 subcultures were used and over 1,500 fermentation tube tests made, resulting in the conclusion that the organisms comprising the group are identical in all morphological, cultural, physical and biochemical features except in ability to ferment dextrose, lactose and saccharose.

An almost complete series of organisms was obtained, showing all except two of the possible combinations of fermentative ability from an organism which regularly produced visible gas in fermentation tubes containing any one of the three carbohydrates mentioned to one which never produced visible gas from either of them. While the final decision as to classification is reserved till work upon the pathogenicity of the various strains or described species is completed, the writers feel that based on the bacteriological studies alone the group should be considered as one somewhat variable species of which *Bacillus carotovorus* Jones is the earliest described and should therefore be considered as the type.

(Data to appear as Technical Bulletin 11 of the New York Experiment Station, and in the Twenty-first Annual Report of the Vermont Experiment Station.)

Timothy Rust in the United States: Mr. EDW. C. JOHNSON, Bureau of Plant Industry.

Timothy rust was reported in the United States by Trelease as early as 1882. Pammel reported it from Iowa in 1891. From 1891 to 1906 no mention of the parasite in the United States has been found. In 1906 the rust became epidemic in the timothy-breeding plats at the Arlington Experiment Farm, Virginia. Since then the rust has been common in many localities. It has been reported from all the states east of the Mississippi and north of Tennessee with the exception of the New England states, New Jersey and Illinois, and from Minnesota and Iowa.

The rust is similar in general appearance and morphological characteristics to *Puccinia graminis* Pers., on wheat. Its æcial stage is not definitely known in this country. Eriksson and Henning, working with a rust on timothy in Sweden, were able to produce æcia on barberries once in nine trials, and that only in one place of inoculation against 92 places inoculated with negative results. In trials in 1895 they again were unsuccessful in 25 inoculations on barberries. They concluded that the rust is a distinct species and named it *Puccinia Phlei-pratensis*. Kern considers it "a race of *Puccinia poculiformis* (*graminis*) or a so-called physiological species."

Inoculation experiments with timothy rust on various grasses in the greenhouses at Washington, D. C., demonstrate that the rust in the United States and the rust in Europe are identical, and that the species is not well fixed. The rust transfers easily to *Avena sativa, Secale cereale, Festuca elatior, Dactylis glomerata, Arrhenatherum elatius* and *Poa compressa*. Inoculations directly on *Triticum vulgare* and *Hordeum vulgare* give negative results.

Timothy plants brought into the greenhouse from Arlington Experiment Farm, Virginia, Jan-

uary 19 and March 12, 1908, began to produce fresh uredospores within six days after transplanting. In the field fresh rust pustules on new growth of timothy were common from March 13 on. Thus the rust mycelium is able to live through the winter in this locality. How the rust winters further north has not been determined. The teleuto stage is more common in Pennsylvania and New York than at the Arlington Experiment Farm, but as the æcial stage is perhaps rare in the United States the occurrence of teleutospores is of doubtful importance.

In timothy-breeding work at the Arlington Experiment Farm in 1908 and 1909, W. J. Morse, of the United States Department of Agriculture, found that the difference in varietal resistance of timothies to rust is well marked. This has also been determined in greenhouse experiments, and, although no variety or strain of timothy has been found to be entirely immune, there is a very noticeable difference in the degree of susceptibility of the different varieties to rust.

Floret Sterility of Wheats in the Southwest: Mr. Edw. C. Johnson, Bureau of Plant Industry.

Floret sterility of wheat, or the non-development of kernels in florets of otherwise normal spikelets, is common in the southwest, especially in parts of Texas and Oklahoma. The trouble has been variously attributed to insects, imperfect fungi, rusts and physiological conditions, but until recently no experiments have been performed to demonstrate what are the principal causes.

In 1908 and 1909 investigations were undertaken at San Antonio, Texas. There the per cent. of sterile florets in wheats was 30 to 50 per cent. and 12 to 15 per cent. for the two years, respectively. Although the exact rôle played by wheat thrips was not established, their importance as agents for spore dissemination was noticed. As many as five rust spores and three conidial spores of imperfect fungi were observed on the antennæ and appendages of a single thrip. As the thrips are exceedingly active and penetrate between the glumes of florets, spores are often carried into the young wheat flowers.

Ovaries of sterile florets were almost invariably affected with fungi. *Cladosporium graminum* Cda., and *Stemphylium* n. sp. were common on the leaves and diseased ovaries of affected grain and rusts, both *Puccinia graminis* Pers. and *Puccinia rubigo-vera* (D. C.) Wint., almost invariably were present in the florets. Inoculation of florets with spores of pure cultures of *Cladosporium graminum* Cda. and *Stemphylium* n. sp. were made by dropping a mixture of spores and water between the glumes held apart with tweezers. This increased the percentage of sterile florets. The increase amounted to three per cent. where 432 florets were inoculated with the former species and 195 florets similarly treated with sterile water were used for control, and 1.9 and 9.19 per cent., respectively, where 186 and 301 florets were inoculated with the latter, and 195 and 198 florets treated with sterile water were used for controls.

Similar inoculations with uredospores of *Puccinia graminis* increased the sterility 21.03 per cent. where 93 florets were inoculated and 206 florets were used for control. In two sets of inoculations where the wheat heads were soaked in water full of spores an increase of sterility of 7.36 per cent. and 6.08 per cent. resulted where 85 florets and 264 florets, respectively, were inoculated and 106 florets and 151 florets similarly treated with sterile water were used for controls.

No precautions were taken to prevent drying of the heads after inoculation, except covering both inoculated and control heads with tissue paper for two days. In the hot, clear days which followed the heads dried very quickly and the per cent. of infection was reduced. In an experiment where the wheat plants were screened from the direct rays of the sun an increase of sterility of 12.32 per cent. above that in adjacent unshaded control plants resulted. No artificial inoculation was performed. Shading prevented rapid drying in the mornings and thus gave better conditions for the development of fungi.

The experiments show that rusts and associated fungi, chief of which is *Stemphylium* n. sp., are undoubtedly the most important causes of floret sterility of wheats in the southwest. That similar conditions often exist in other localities was demonstrated at Minnesota in 1909. In the plats for rust resistance breeding all the grains when in bloom were sprayed with rust spores. In all the non-resistant wheats a large per cent. of the florets produced no kernels on account of rust infection in the heads, while in adjacent unsprayed plats such sterility was not marked.

Bacterial Blight of Mulberry: Dr. Erwin F. Smith, Department of Agriculture.

In 1890 Cuboni and Garbini studied a disease of the mulberry about Verona. This was ascribed to a *Diplococcus* believed to be identical with or akin to *Streptococcus bombycis*, supposed to be the cause of a disease of silk-worms. Successful

inoculations were claimed. In 1891–92 Macchiatti published papers on the disease, confirming the views of Cuboni and claiming successful inoculations. In 1894 Boyer and Lambert, in France, studied a blight of mulberries, obtained inoculations from cultures, and named the organism *Bacterium mori*, but did not describe it. In 1897 Peglion confirmed Macchiatti's views, obtained infections on leaves and shoots in three days' time, and stated the organism to be yellow and a liquefier of gelatin. Possibly he was working with mixed cultures.

In 1905 the writer made isolations from blighting mulberry leaves, and, influenced by the Italian work, paid attention only to such poured-plate colonies as were distinctly yellow. Two yellow forms were isolated and thorough inoculations were made on growing leaves and shoots of mulberries, but, contrary to expectation, no trace of infections was obtained. The diseased material came from Georgia.

In 1908 plates made from Georgia material showed the bulk of the bacteria in the freshly blighting stems to be a white species. With this white organism numerous successful infections were obtained on two varieties of mulberry, on both leaves and stems. With pure cultures plated from such blighting shoots, many additional infections were obtained. Independently at about the same time two of my co-workers obtained confirmatory results with the same white organism: (1) isolations and successful inoculations on the Pacific slope by Mr. P. J. O'Gara (oral communication); (2) isolations and successful inoculations in Arkansas by Mr. James Birch Rorer (oral communication). Typical-looking cultures were received from both men and with the Arkansas organism successful inoculations were made in a Department of Agriculture hothouse under my direction and also by Mr. Rorer himself. There is, therefore, no doubt whatever as to the infectious nature of the white organism. Whether the Italians who have secured infections inoculated with mixed cultures, one constituent of which was this white organism, or whether there is also a yellow organism (*Bacillus Cubonianus* Macch.) capable of causing a bacterial blight of mulberry, must be left an open question. If the latter supposition be true then *Cubonianus* is perhaps the proper specific name for the yellow organism.

Inasmuch as Boyer and Lambert obtained infections with their *Bacterium mori*, and have not made any incorrect statements respecting its character, I have adopted their name for the white organism, with the following emended characterization:

Bacterium mori B. & L. emend. Schizomycete causing a blight of leaves and young shoots of the mulberry. Spots at first water-soaked, then, sunken and black; foliage more or less distorted; shoots soon show sunken, black stripes and dead terminal portions. Action of disease rather prompt. In very young shoots all the tissues are involved—wood, pith and bark being infested by the bacteria. In older shoots the bacteria are confined mostly to the xylem and especially to the vessels, where tyloses are produced, as a result of the stimulus of the organism.

The organism is motile by means of a polar flagellum, sometimes two are present. It is actively motile when examined in a hanging drop made from a three-day agar culture. It occurs as single rods, pairs and short or long chains. The ends of the rod are rounded and the limits of size are 1.8 to 4.5 $\mu \times 0.9$ to 1.3 μ. Most are 3.6 $\mu \times 1.2$ μ. No spores have been observed. Pseudozoogloeæ occur, and involution forms were seen in beef-bouillon containing 6 per cent. sodium chloride. It stains readily with carbolfuchsin, but not by gram.

Colonies on + 15 Agar at 23° C.—White, slow-growing, round, smooth, flat, edge entire becoming undulate after some days, internal structure reticulate or striate.

Young Agar Streaks.—Growth moderate, spreading, flat, dull, smooth, becoming finely granular, translucent, slimy, odorless, white, medium not stained.

Agar Stabs.—Best growth at top.

Potato.—Growth moderate, spreading, flat, glistening, smooth, white to dirty white, slimy and medium grayed, only slight action on the starch.

Lœffler's Blood Serum.—Streak spreading, flat, glistening, smooth, white. No change in color of substratum or liquefaction (two months).

Surface Colonies on + 10 Nutrient Gelatin.—Flat, slow-growing, round to irregular, with lobate-erose margins.

Gelatin Stabs.—Best growth at top, line of stab filiform, no stain, no liquefaction.

Peptonized Beef-broth (+ 15).—Produces a pellicle, which breaks into fragments readily and sinks, forming a flocculent fluid; strong turbid clouding (clear after three months). Growth always best at the top, no distinct odor.

Milk.—Coagulation absent, fluid becomes clear

by destruction of the fat. After three months, and considerable evaporation, the fluid is more or less gelatinous and somewhat brownish (the ochraceous to ochraceous-buff of Ridgway, and near the ochroleucous of Saccardo). In such cultures there is always a small amount of pure white bacterial precipitate and the microscope shows entire absence of fat globules. Such milk is translucent, strongly alkaline and not viscid. At no time does the culture show any acid reaction or any striking reduction of litmus. Purple litmus milk blues promptly.

Cohn's Solution.—No growth, or very scanty.

Uschinsky's Solution.—Copious growth, not viscid, heavy fragile pellicle, sinking readily. Fluid bluish-fluorescent as early as the fifth to tenth day.

Sodium Chloride.—Tolerates 6.5 per cent. sodium chloride in + 15 peptonized beef-bouillon. It also grew twice in presence of 7 per cent. sodium chloride, but failed once when less copiously inoculated and did not grow in 9 per cent. sodium chloride bouillon.

Chloroform.—Grew unrestrainedly and for a long time in bouillon standing over chloroform.

Fermentation Tubes.—Does not produce gas or cloud closed arm in peptone water containing any of the following carbon compounds: dextrose, cane-sugar, milk-sugar, maltose, glycerine or mannit. Strongly aerobic.

Indol Production.—Absent or feeble.

Nitrites.—Nitrates not reduced to nitrites in b-ef-bouillon.

Temperature Relations.—Thermal death point about 51.5° C. Maximum temperature for growth about 35° C. Remains alive only a short time at this temperature. Minimum temperature for growth below 1° C.

Drying.—Rather resistant on cover-glasses—alive after 30 days, and another time after 50 days.

Sunlight.—Sensitive. Exposed in thin sowings in + 15 nutrient agar in Petri dishes bottom up on ice, one half of each plate covered, seventy per cent. were killed by 15 minutes' exposure, one hundred per cent. by 35 minutes' exposure, and ninety-five per cent. by 25 minutes' exposure. Colonies on the covered side developed freely.

The following are recommended as quick tests for differential purposes: Pitfield's flagella stain, peptonized beef-broth, Uschinsky's solution, Cohn's solution (5 days), litmus milk, nitrate bouillon, sodium chloride bouillon (5 per cent.), gelatin and agar plates; inoculation by needle-puncture into young rapidly growing shoots of susceptible species of Morus, which should show water-soaked spots in 7 days or less.

A New Spot Disease of Cauliflower: Lucia McCulloch. (Read by title.)

A New Tomato Disease of Economic Importance: Dr. Erwin F. Smith, Department of Agriculture.

In the summer of 1909 my attention was called to a stem disease of tomatoes prevalent in the vicinity of Grand Rapids, Mich. Microscopic examinations showed absence of fungi and great numbers of bacteria with considerable destruction of the inner tissues. Petri-dish poured-plates were made from these stems and the organism occurring in the plates proved to be a yellow schizomycete. Inoculations were made on July 27 in the open with material taken directly from the stems and shaken in bouillon, and the disease (gross appearance and histological phenomena) was in this way reproduced in a number of large tomato plants, progressing slowly, however. Poured-plates made from the interior of these plants demonstrated the presence of the same yellow organism in enormous numbers and another series of inoculations was made in October in one of our hothouses, using sub-cultures from typical colonies on these poured-plates. The results were the same as in case of the direct inoculations—all the plants contracted the disease, became stunted and were finally destroyed by it, but its progress was relatively slow, one or two leaves at a time slowly wilting or yellowing and shriveling; in other words, there is not that sudden collapse of the whole plant so characteristic of the southern bacterial disease of tomatoes (photographs were passed about showing various stages of this disease as obtained by pure culture inoculations).

The bacteria are very abundant in the vascular bundles, but the brown staining is less pronounced than in case of the disease due to *Bacterium solanacearum*.

The bacteria occur in the vascular system, but also hollow out cavities in pith and bark. The foliage is stunted and becomes yellowish, one leaf and one branch after another slowly succumbing to the disease. I am not sure whether the disease begins above ground or below. Whether the fruit itself shows the bacterial infection or not must also be left an open question. In the field, tomatoes from such plants were frequently brown spotted, but the origin of this brown spotting is still in some doubt.

[Since the above paragraph was written many of our check tomatoes in hothouses have contracted the disease, also much younger tomato plants on neighboring benches, together with a purple-flowered spiny Porto Rican weed (*Solanum globiferum*?) grown in the house because of its reported resistance to the brown rot. Not in a long time have we had such a wholesale escape of a bacterial disease to our check plants, and the indications are that the disease is readily communicated from plant to plant through the parts above ground, this being favored by liberation of the bacteria through the frequent cracking open of the diseased stems. We have also found the bacteria abundant in the fruits of diseased plants.]

The losses around Grand Rapids, Mich., last year amounted to eight or ten thousand dollars, and the writer has some evidence indicating that the disease is prevalent in other parts of the northern United States, and has probably hitherto been confused with the more rapidly acting disease due to *Bacterium solanacearum*. I suspect it to be a disease of hothouses as well as of the open.

Only some preliminary notes can be offered at the present time on the cultural characteristics of this organism, which may be known as *Bacterium* (?) *Michiganense*. Some of these characters are as follows:

The organism when taken from the vessels is a short rod with rounded ends, single or in pairs, termo-like; taken from ten-day agar culture and stained with carbol fuchsin, the majority are 0.35 to 0.4 × 0.8 to 1.0 μ. The writer observed no active self-motility when taken from the stem or old agar-cultures and examined in water. On staining young agar-cultures for flagella they appeared to be polar, but no good preparations were secured.

In morphology, as taken from the stem, the organism closely resembles *Bacterium solanacearum* as it occurs in the southeastern part of the United States. The organism from the stems came up rather slowly in + 15 agar-plates, the first colonies to appear being a few scattering intruders. Afterwards the right organism appeared plentifully in the form of pale yellow, smooth, wet-shining, round surface colonies not unlike those of *Bacterium campestre*. The buried colonies were small, round to broadly elliptical. The intruders in this case formed wrinkled, raised, gummy-looking, roundish yellow colonies.

Agar Stabs.—Surface growth in 15 days, at 25° C., 10 mm. in diameter, canary yellow, smooth, shining, opaque, flat, viscid. Stab growth finely saccate. Grows slowly on agar.

Corn-meal Agar Stabs.—Scanty, pale yellow surface growth. Moderate stab growth; better than in peptonized beef-agar.

Potato Cylinders.—After a month's growth moderate, spreading, thin, smooth, canary yellow; moderate amount of yellow precipitate in the liquid which is clear, *i. e.*, not thickened; potato slightly browned. This serves to distinguish the organism from *Bacterium campestre* and *Bacterium phaseoli*. The potato becomes alkaline to litmus paper. Only a small portion of the starch is destroyed.

Nitrate Bouillon.—Does not reduce nitrates to nitrites.

Cohn's Solution.—No growth.

Milk.—After fifteen days the surface of the milk is yellow (canary yellow to a depth of 3 to 4 mm.). There is also a yellow rim 2 to 3 mm. wide. In the lower part of the tube the milk was cream color, and was not solidified. The yellow layer on the surface increased in depth until at the end of a month it was 10 to 12 mm. in depth and yellow, the milk below having become a deep cream color, thick and smooth like butter. At this time there was some yellow precipitate at the bottom of the tube. In another set of test-tube cultures the milk at the end of fifty days showed a yellow translucent whey 12 to 25 mm. in depth, the curd being deep cream color. There is probably a lab ferment.

Litmus Milk.—The litmus is reduced. At the end of fifteen days the medium was uniformly pale gray (Saccardo's griseus) and liquid throughout. After a month the litmus color had nearly all disappeared, the milk being dirty cream color and somewhat thickened.

Beef Bouillon.—The appearance at the end of fifteen days was as follows: Moderate clouding, thin white flocculent masses suspended in the medium. A moderate slimy precipitate, which rises in long strings on whirling; these break with shaking, but do not readily dissolve. No rim or pellicle. After another three weeks, rolling clouds, densest at surface, wide patches of rim, no pellicle; precipitate moderate, yellowish, viscid, rises in a swirl on whirling. Organism grows slowly in + 15 bouillon.

Gelatin Stabs.—Growth after five weeks scant, canary yellow, surface smooth, shining, slight in the stab, no liquefaction (temperature 14° to 15° C.).

Very little is yet known respecting the methods of natural infection or the period of incubation. I am inclined to think, however, that the infection takes place several weeks before there is any general indication of the disease in the fields, and possibly dates from the time of transplanting.

Sulphur Injury to Potato Tubers: Mr. W. A. Orton and Miss Ethel C. Field, Bureau of Plant Industry.

This paper is the outgrowth of experiments conducted in California in 1909 for the control of potato scab. Among other substances flowers of sulphur was used in varying quantities to disinfect soils where the scab fungus was present. On digging the crop, many tubers from the sulphured rows showed sunken, dark spots from 5 to 30 mm. in diameter, which were relatively free from fungous or bacterial infection. These spots occurred only in tubers from sulphured rows. They were more numerous in the heavily sulphured plots, but were present even where the seed piece had merely been dipped in sulphur. Potatoes exposed to sulphur fumes in the laboratory developed similar depressed spots.

This injury has apparently not been observed in the sulphur experiments conducted in the east. The California soils are peat and in late fall became quite dry near the surface, so that volatilization of the sulphur could easily have occurred.

Outbreak of Potato Canker (Chrysophlyctis endobiotica Schilb.) in Newfoundland, and the Danger of its Introduction into the United States: Dr. H. T. Güssow, Central Experimental Farm, Ottawa.

This well-known European potato disease has been recognized in specimens which I received from Red Island, Placentia Bay, N. F. The disease is due to a fungus of the order Chitridineæ and was named by its discoverer, Professor Schilbersky, in 1896, *Chrysophlyctis endobiotica*. The fungus attacks the tubers, but cases have been observed where the leaves closely above ground were also attacked. The changes due to the fungus on the tubers are very characteristic. Unfortunately the disease is not noticeable in the field until the crop is harvested, when it will be shown that the tubers are covered—according to the severity of the attack—either at the eyes only, or half or wholly by peculiar excrescences, not unlike the common crown galls of fruit trees. When a tuber is wholly covered with these excrescences they have lost all resemblance to potatoes and appear like irregular lumps of clay or coke. The fungus lives in the cells of these excrescences, which are not covered by the epidermis. It is present in these cells, first, as a more or less free plasmodium; second, as hyaline globular bodies, enclosed by a thick membrane and third, as yellowish brown resting spores very similar in appearance to those of the Peronosporæ. This latter stage is the most common one. The spores are very difficult to germinate artificially. Successful germination test showed that the spores burst and numerous swarm spores were liberated. These swarm spores infect new cells passing through the different stages—all of which are unsatisfactorily known—indeed it is doubtful whether there is any justification for the new generic name as described. The tubers decay by the action of the parasite and when harvested break to pieces and thus the soil becomes infected. The disease made its appearance in 1901 in England, is now present in Ireland, Scotland, Scandinavia, Germany and other European countries, but was not, until its discovery in Newfoundland, known on this side of the Atlantic. A visit to Newfoundland led to the discovery of the disease all over the neighborhood, and subsequently it was found to exist in other localities as well. As it was pointed out to me on inquiry that potatoes were imported in small quantities to the United States and Canada, great precaution is necessary to prevent the introduction and establishment of this serious pest. On account of the dangerous nature of the disease it was recommended that immediate action should be taken to safeguard the interest of the American and Canadian farmers, and a committee be appointed to consider the best means of dealing with the possible danger from its introduction into the United States. The fungus has also been referred to as *Œdomyces leproides* Trabut, but it is very different from this fungus, which according to Magnus is synonymous with *Synchytrium putwosam*.

Rhizoctonia Stem Rot of Beans: Mr. M. F. Barrus, Cornell University.

While working on bean diseases in the vicinity of Oneida, N. Y., during the summer of 1909, quite a large percentage of plants were noticed to be affected with a disease which caused cankers on the parts of the stem below or at the surface of the ground, these lesions frequently encircling the stem, causing it to break over and resulting in the death of the plant. In some fields as much as 30 per cent. of the plants became thus affected.

During the following season at the same place the disease was found to be as prevalent as it was the year before. In some fields it caused the death of at least 5–6 per cent. of the seedlings, and, later in the season after a rainy spell, a large percentage of the pods in contact with the ground became infected.

When diseased stems or pods were placed in a moist chamber over night a fine moldy growth surrounded them. Direct cultures made from the stem gave a pure culture of a fungus, which, from the character of mycelium and the production of sclerotia, showed that it belonged to the form genus *Rhizoctonia*. Interesting studies were made of its growth on various media. Inoculation of healthy plants grown in sterile soil resulted in the production of lesions characteristic of the disease, upon the inoculated plants, the checks remaining healthy. Subsequently from these lesions the fungus was again isolated and the characters of its growth noted. Inoculations were also made on healthy pods, in every case resulting in a characteristic *Rhizoctonia* canker. No perfect stage has yet been observed.

The writer is carrying on further experiments with this organism and with a culture of *Corticium vagum* in an effort to discover whether they are identical. Professor H. R. Fulton, formerly of the Louisiana Agricultural Experiment Station, carried on a considerable number of infection experiments during the summer of 1907 with a *Rhizoctonia* which he isolated from the bean pod, and produced lesions on seedling beans and on injured pods.

Observations on Apple-tree Anthracnose: Professor H. S. JACKSON, Oregon Agricultural College and Experiment Station. (Read by title.)

The Frog-eye Disease of Apple Leaves: Dr. JOHN L. SHELDON, University of West Virginia.

The history, cause and present distribution of this destructive disease of apple foliage are referred to briefly. Several reasons are given why it seems preferable to use the name "frog-eye" for the disease of apple leaves caused by *Illosporium malifoliorum* instead of the name "brown-spot." (Specimens of the diseased leaves were shown.)

The Ohio Outbreak of Fusarium Blight of Potato in 1909: Professor A. D. SELBY, Ohio Agricultural Experiment Station. (Read by title.)

On Mutualism in certain Parasitic Bacteria and Fungi: Mr. THOS. F. MANNS, Ohio Agricultural Experiment Station.

In artificially demonstrating the production of disease, the writer believes that in the past too little recognition has been given to the organisms associated with the specific cause of the disease. It seems quite probable that the intensity of the disease, together with the varying symptoms, depends quite largely upon the parts played by others than the specific organism. In past experimental work on disease production, we have proceeded by determining the specific organism and eliminating all the associated organisms. The writer believes that in the future, if we are to know more concerning the progress of disease and the cause of its virulence, we must take into account the rôle played by the intimately associated organisms.

During the past two years the writer has been working upon the blade blight or "red leaf" of oats; a disease which experimentally is shown to be due to bacteria. In this work two bacteria were associated in the diseased blades. Inoculation work with each of the organisms separately showed that one was specific and capable of producing limited lesions in the oat blade, while the second organism produced no lesions at all; however, when both the organisms were inoculated together as a mixture the typical oat blight symptoms followed. After repeated demonstrations with similar results, it was concluded that we have in these two organisms a mutualism or symbiosis in the production of this disease. Platings from the inoculation of the two organisms in mixture showed the presence of both the organisms throughout the resulting lesions. The writer has described the specific organism as *Pseudomonas avenæ* n. sp. and the associated organism as *Bacillus avenæ* n. sp.

On artificial media considerable advantage was noted in the growth and virulence of the specific organism when grown with the associated organism.

The writer believes there exists similar relationships among fungi in the production of disease, however, in these cases, the associated organism may be only a semiparasite, following closely on the heels of the specific organism. It seems probable also that such relationships as the latter may exist between the specific fungus and certain bacteria.

Such relationship suggests itself as prevailing between the *Fusarium* of potato wilt and a certain *Vermicularia* which is so frequently associated in culture work upon potatoes infected internally

with the *Fusarium*. Through artificial culture work it was found that 62.8 per cent. of the tubers from a certain field was infected internally with the *Fusarium*, along with which was also the *Vermicularia* to an extent of 10.3 per cent. Culture work upon beginning lesions in the stem and roots usually brought out both of the fungi.

No experiments have been carried out to show whether both these organisms are actually taking part in the production of potato wilt, although such experiments are now under way.

On a Laboratory Method of Determining the Fungicidal Value of a Spray Mixture or Solution: Dr. Donald Reddick and Mr. Errett Wallace, New York State College of Agriculture.

The method consists essentially of spraying slides or cover-glasses with a spray substance of a given formula. After proper drying and exposure spores of the pathogen are placed on them in a drop of meteoric water to germinate. This method more nearly simulates natural conditions than that of using a drop of the spray substance direct. Experimental data in connection with the conidia of *Venturia inæqualis* have been obtained which confirm the fact.

Mycological Studies upon Wheat and Wheat Soils to Determine Possible Causes in Deterioration in Yield: Professor T. D. Beckwith, North Dakota Agricultural College and Experiment Station. (Read by Professor H. L. Bolley.)

Analysis of soil solutions made from old wheat soil and from virgin prairie soil did not show sufficient differences to warrant the assumption that deterioration in yield is due to lack of plant food.

Culture studies made from old wheat soil and from virgin prairie soil show that certain soil fungi belonging to genera known to be pathogenic to some of the gramineæ are present in the soil cropped for years to wheat. They are almost lacking in virgin soil, the probabilities being that they are wind sown.

These fungi belong to the genera *Colletotrichum*, *Fusarium*, *Macrosporium* and *Alternaria*.

In order to ascertain whether spores of certain of these fungi were normally to be found on wheat stems a series of four hundred germination tests were carried out by placing them in moist culture tubes. Examination was made microscopically after five days' incubation at 30° C. Following are the results showing the percentages of wheat infected by these fungi:

Nodes	
Colletotrichum	90.0
Macrosporium	65.0
Helminthosporium	62.5
Cephalothecium	10.5
Internodes	
Colletotrichum	83.0
Macrosporium	50.5
Helminthosporium	58.5
Cephalothecium	9.0

This preliminary series showed the possibilities for infection. The spores of these forms either were resting on the wheat plants or else had already germinated there.

The next series consisted of another four hundred nodes and internodes, but this time they were sterilized by treating one minute with one per cent. formaldehyde and afterward washing with sterile distilled water. Thus it is presumed that all saprophytes and surface fungi were eradicated. These stems were then allowed to germinate as in the former series. Microscopic examination showed the following per cent. infection by the fungous genera given below:

Nodes	
Colletotrichum	57.0
Macrosporium	53.5
Helminthosporium	40.5
Fusarium	33.5
Internodes	
Colletotrichum	52.5
Macrosporium	33.0
Helminthosporium	34.5
Fusarium	27.5

Finally culture experiments made from roots of wheat grown in old wheat soil showed the presence of *Colletotrichum*, *Fusarium* and *Macrosporium*.

These tests seem to prove (1) old wheat soil is infected with certain fungi, (2) the spores or mycelium of certain of these fungi are to be found normally in or on the wheat plant grown on such land, (3) a certain per cent. of the wheat is pathologically infected with certain of these fungi, (4) certain of these fungi cause root infection.

Peach Yellows and Frost Injury: Mr. M. B. Waite, U. S. Department of Agriculture. (Read by C. L. Shear.)

There seems to be some confusion about these two troubles of the peach. It is the writer's opinion that peach yellows has no relation what-

ever to winter injury. Peach yellows is thought by the writer to be a contagious disease, though the germ has never been discovered. It behaves in many ways, though not in all respects, like pear blight. For example, when the pear blight germ is absent from a locality there can be no blight no matter how favorable conditions may be. In the same way peach yellows has a distinct range in the northern and eastern part of the United States. It has increased its area rather rapidly. No matter what the conditions may be of soil, climate, method of culture, fertilizer, etc., when the yellows reaches a district it attacks the orchards.

Pear blight has its ups and downs. Some years the conditions are favorable and some years unfavorable for the spread of the disease. Peach yellows behaves in the same way. Pear blight spreads from colonies or infection centers. Peach yellows behaves in exactly the same way.

Pear blight lives over winter in the "hold-over" cases, this becoming the new infection centers each spring. With peach yellows every case is a hold-over till the tree dies.

Pear blight can be inoculated artificially by introducing the germ or the diseased tissues. Peach yellows can be inoculated by introducing a bit of living tissue. Both diseases are unknown elsewhere in the world, although their host plants are foreign to this country and are cultivated widely over the earth.

Pear blight was mistaken for frost injury before its bacterial nature was discovered.

We know peach yellows as a distinct disease, through a number of definite symptoms. The distinctive symptoms of peach yellows are, first, the premature, red-spotted fruit; second, wiry or bushy vertical sprouts of a peculiar character. Peach yellows has also certain leaf symptoms, such as yellowing and curling. These symptoms are also shared by the disease known as "little peach." The leaf symptoms, however, are not entirely reliable, as somewhat similar symptoms, often difficult to distinguish, are produced by winter injury to various parts of the trunk, collar and root, the peach borer, the root aphis, sour soil, chlorosis, or even nitrogen starvation or soil poverty.

Frost collar girdle may even produce slightly premature fruit as other girdling will do, but it is not typical, for the yellows and the symptom would not be reproduced in budding. True yellows is often mixed up in the same orchards with frost injury and other similar confusing symptoms. Oftentimes, however, through examination of doubtful trees there will be found other symptoms than yellows.

Frost injuries, particularly, since 1903 and 1904, occurred from Michigan to New York and New England in the yellows area. The eastern part of the frost injury area overlaps a district in which there has been an extensive outbreak of yellows. This district extends from New England, eastern and southern New York to Tennessee and North Carolina. Frost injury has been severe without accompanying yellows in western New York, Ohio and Michigan. Yellows has been severe without frost injury in New Jersey, Delaware, Maryland, southern Pennsylvania to Tennessee and North Carolina. The overlapping of these two troubles in southern New York and New England need not, therefore, be confusing.

C. L. SHEAR,
Secretary-Treasurer

SOCIETIES AND ACADEMIES

THE CHEMICAL SOCIETY OF WASHINGTON

THE 198th meeting and annual smoker was held at Fritz Reuters on Thursday, April 14. The attendance at the smoker, which consisted of a beefsteak dinner, was 57. The following papers were read at the meeting:

The Effect of Drugs and Diet upon the Thyroid: REID HUNT.

Dr. Hunt discussed the changes in resistance of animals to certain poisons caused by the administration of various iodine compounds. Evidence was presented that some of these changes are caused by an effect upon the thyroid gland and that certain iodine compounds have a selective action upon this gland, that, in other words, they are thyreotropic. Diet also was found to have marked effects upon resistance to certain poisons; some of these effects seem to be exerted, at least in part, through the thyroid gland.

Contribution to the Knowledge of Phosphoric Acid: B. HERSTEIN and LYMAN F. KEBLER.

Dr. Herstein said, in part, that a method having been found to determine each of the three hydrates of phosphorus pentoxid, when mixed with one another, commercial glacial phosphoric acid and metaphosphoric acid as prepared in the laboratory, were subjected to a study, the results of which showed that: (1) contrary to the hitherto accepted theory, metaphosphoric acid in changing to the ortho-form first becomes pyrophosphoric acid; (2) the percentage rate of inversion is very little, if at all, influenced by dilution.

Extensive tables and diagrams were prepared in support of the above.

Separation and Determination of Cocain and Strychnin, and Atropin and Strychnin when they Occur Together: H. C. Fuller.

Mr. Fuller explained that the alkaloids are extracted from the drug product and weighed together, using proper precautions to obtain them in a pure condition. They are then dissolved in alcoholic potash, transferred to a presure flask and heated over the steam bath for one hour, which completely hydrolyzes the cocain and atropin, but does not affect the strychnin. The latter is then separated and weighed.

J. A. LeClerc,
Secretary

THE AMERICAN MATHEMATICAL SOCIETY

The one hundred and forty-eighth regular meeting of the society was held at Columbia University on Saturday, April 30. The attendance at the two sessions included forty-two members. Ex-President W. F. Osgood occupied the chair at the morning session, Ex-President T. S. Fiske and Professor Frank Morley at the afternoon session. The council announced the election of the following persons to membership in the society: Mr. F. W. Beal, Princeton University; Professor W. J. Berry, Brooklyn Polytechnic Institute; Mr. J. K. Lamond, Yale University; Mr. R. M. Mathews, University High School, Chicago, Ill.; Professor F. E. Miller, Otterbein University; Mr. J. E. Rowe, Johns Hopkins University; Mr. W. H. Terrell, Clyde, N. C.; Mr. George Wentworth, Exeter, N. H.; Mr. W. A. Wilson, Yale University. Eight applications for membership in the society were received. The total membership is now 630.

Professor Maxime Bôcher was elected a member of the editorial board of the *Transactions*, to succeed Professor W. F. Osgood at the expiration of the latter's term of office. Professor L. E. Dickson was appointed to fill the unexpired term of Professor E. B. Van Vleck, who retires from the board in July.

The committee of publication was directed to publish in book form the lectures delivered at the Princeton Colloquium in September, 1909, by Professors G. A. Bliss and Edward Kasner. The Yale Colloquium lectures have just appeared from the press of Yale University.

The following papers were read at the April meeting:

H. B. Phillips: "Application of Gibbs's indeterminate product to the algebra of linear systems."

H. B. Phillips: "Concerning a class of surfaces associated with polygons on a quadric surface."

Virgil Snyder: "Conjugate line congruences contained in a bundle of quadric surfaces."

W. B. Carver: "Ideals of a quadratic number field in canonic form."

G. A. Miller: "On a method due to Galois."

E. H. Taylor: "On the transformation of the boundary in conformal mapping."

W. B. Fite: "Concerning the invariant points of commutative collineations."

R. G. D. Richardson: "On the saddle point in the theory of maxima and minima and in the calculus of variations."

H. H. Mitchell: "Note concerning the subgroups of the linear fractional group $LF(2, p^n)$."

H. H. Mitchell: "The subgroups of the linear group $LF(3, p^n)$."

C. L. E. Moore: "Some infinitesimal properties of five-parameter families of lines in space of four dimensions."

Edward Kasner: "Forces depending on the time, and a related transformation group."

F. H. Safford: "Sturm's method of integrating $dx/\sqrt{X} + dy/\sqrt{Y} = 0$."

G. F. Gundelfinger: "On the geometry of line elements in the plane with reference to osculating circles."

The Chicago Section of the society held its spring meeting at the University of Chicago, April 8–9. The summer meeting of the society will probably be held in New York City early in September.

F. N. Cole,
Secretary

THE AMERICAN CHEMICAL SOCIETY
RHODE ISLAND SECTION

The regular March meeting of the section was held March 31, 1910, at the University Club, preceded by the usual informal dinner. Professor William H. Kenerson, of the engineering department of Brown University, presented the paper for the evening on the subject, "Some Problems of the Testing Laboratory." The speaker showed by means of lantern slides the various types of testing machines and explained their method of operation and the results obtained. Then he took up some of the special problems that had been presented to the Brown Laboratory and showed the methods and machines devised to secure accurate results in the solving of these unusual cases.

Albert W. Claflin,
Secretary

Providence, R. I.